THE
MEDICAL AND SURGICAL
REPORTER.

No. 2076. SATURDAY, DECEMBER 19, 1896. VOL. LXXV—No. 25

ORIGINAL ARTICLES.

THE LOCAL APPLICATION OF COLD IN ACUTE PNEUMONIA. BEING THE THIRD COLLECTIVE REPORT, WITH A DISCUSSION OF FEVER IN THIS DISEASE.*

THOMAS J. MAYS, A.M., M.D.,† PHILADELPHIA.

A little more than a year ago I had the honor of presenting to this Society my second collective report on the influence of cold in the treatment of pneumonia, which, with previous collections, comprised the histories of one hundred and ninety-five cases of this disease so treated, and this evening I beg leave to make a further contribution of one hundred and four cases to the same subject—making a total of two hundred and ninety-nine cases of acute pneumonia managed in this way. To the medical journals that again kindly published my late announcement inviting contributions, and to the medical gentlemen who so generously and liberally responded to my appeal, I desire to extend my grateful thanks. Of the one hundred and four cases, which are here given in tabular form, only ninety-four are reported fully enough to give the sex, age, highest temperature attained; highest number of respirations; number of days from rigor to crisis, or subsidence of fever; presence or absence of delirium; whether single or double pneumonia, recovery or death; and name and address of each reporter. In a few of the cases not all these data were furnished.

*Read before the Philadelphia County Medical Society, December 9, 1896.

†Professor of Diseases of the Chest in the Philadelphia Polyclinic, and Visiting Physician to the Rush Hospital for Consumption in Philadelphia.

TABULAR VIEW OF NINETY-FOUR CASES OF ACUTE PNEUMONIA.

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and Addresses of Reporters.
1	F	5½	105	.....	6	No	S	R	Well and out of bed on seventh day.	M. V. Ball, Philada.
2	M	4	104	.....	8	No	S	R	Very serious case; cyanosed; oxygen used.	" "
3	F	5	104	30	9	Yes	S	R	Recovery was prompt.	" "
4	F	8	104	.....	4	No	S	R	"In my cases ice was well borne, allayed cough, and recovery, as a rule, was very prompt."	" "
5	M	3	104.8	52	5	Yes	S	R	Catarrhal pneumonia following measles.	Chas. E. Woodward, West Chester, Pa.
6	M	3	105.2	50	3	Yes	D	R	Catarrhal pneumonia following measles; convulsions for twenty-four hours. Both of these cases were "promptly brought under control by the application of ice."	" "
7	M	27	104	40	2	No	S	R	Resolution progressive and permanent.	Henry E. Tuley, Louisville, Ky.
8	F	9	.....	....	2	Yes	S	R	Crisis occurred twenty-four hours after ice was applied.	R. E. Buchanan, Independence, Ia.
9	F	25	.. ....	....	.....	Yes	S	R	"I now use cold applications in all pneumonias and eruptive fevers, with uniform success."	" "
10	M	7	.....	.....	3	Yes	S	R	Relief was prompt.	" "
11	M	38	105.4	36	8	Yes	D	R	Very critical attack in an alcoholic and apparently hopeless case. Two hours' application of ice brought the temperature down from 105.4° to 101°.	P. F. Metz, New Haven, Conn.
12	M	75	105.5	40	4	Yes	S	R	Third attack of pneumonia within two years.	" "
13	F	36	104	32	5	Yes	S	R	As in the other cases ice relieved dyspnea, pain in chest and restlessness.	" "
14	M	10	104.5	44	4	Yes	S	R	Ice gave instant relief to pain in chest.	" "
15	M	58	105.5	40	4	Yes	D	R		" "
16	M	11	105	40	2	Yes	S	R	Discharged on sixth day.	" "
17	M	30	105.2	44	3	Yes	S	R	An alcoholic. Expectoration bloody at first.	" "
18	F	65	105.5	40	3	Yes	S	R	Reporter applied ice in a bag, mixed with saw-dust and covered with oiled silk.	" "
19	M	6	104	.....	.....	Yes	S	R	Pneumonia following measles.	A. H. Lindorne, Atlanta, Ga.
20	M	32	105	35	7	Yes	D	R	Case of alcoholic pneumonia with wild delirium tremens for three days and nights, which made it necessary to strap his four extremities to the four corners of the cot. The marked cyanosis was relieved by oxygen. Internally gave strychnin and capsicum in large doses.	Thomas J. Mays, Philadelphia.
21	M	35	103.5	32	8	Yes	S	R	A severe case.	Wm. A. Kimmil, Chicago.
22	M	14	104	.....	.....	Yes	S	R	Cloths, wrung out of ice water, applied to chest.	Geo. F. Dunham, Wenona, Ill.
23	F	2⅓	104	60	5	Yes	D	R	Had trouble in inducing family to use ice at first, but it acted "marvelously" when it was applied in this case.	Ira Haynes, Nashville, Tenn.
24	M	12	.....	.....	.....	.....	S	R	Asthenic case. Objections to the use of ice by family. These were overcome.	" "
25	F	10	.....	.....	5	No	S	R	Sister of last case. Same objections to ice. Cold reduced temperature in a short time.	" "
26	M	23	105	36	5	Yes	S	R	Treatment very satisfactory. Patient had pneumonia three years before, which lasted for two weeks.	" "
27	M	2½	103	.....	5	No	S	R	This case was a brother to the following one.	" "
28	F	5	103	60	6	Yes	S	R	Reporter says ice treatment is as good for children as for adults.	" "
29	F	2¼	105	80	6	Yes	S	R	Case with "most unfavorable prognosis." Ice was not applied until the fourth day, and then as a last resort, and reporter believes "it saved the child's life."	G. H. Franklin, Hightstown, N. J.
30	F	10 mos.	104	.....	.....	.....	...	R	Patient became progressively worse under hot poultice treatment. Ice was substituted and temperature fell from 104° to 102°.	Charles Rea, York, Pa.
31	F	76	.....	.....	.....	.....	...	D	Pneumonia complicated with influenza. Dr. Rea reports FOUR other cases (not tabulated) treated with cold, the ages of three of which were below eight, and one sixty years, of which he has no record in detail, but with recovery in all. Two of these were seriously complicated with pertussis. In conclusion he says that these cases "might have gotten well without the cold treatment, yet, apparently, I got better results *and my patients were more comfortable* than when I used other forms of treatment."	" "
32	M	4	105	60	6	Yes	S	R	Applications of cold to chest reduced the temperature and respiration-frequency immediately.	Anna M. Stuart, Elmira, N. Y.
33	F	14	106.2	40	5	Yes	S	R	Also pleural effusion. Aspiration.	H. G. McCormick, Williamsport, Pa.
34	F	58	103.5	36	3	Yes	S	R	Ice was applied when fever was highest, and the temperature fell at once and gradually came to a normal point.	" "
35	F	24	103.1	40	9	No	D	R	Case in a delicate girl. Ice gave decided comfort. In reporter's opinion recovery is due to the cold application.	J. H. Wilson and H. S. McConnel, New Brighton, Pa.
36	M	56	.....	.....	.....	No	D	R	Reporter says: "I most thoroughly believe in this form of treatment."	Theoda Wilkins, Pomona, Cal.

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and addresses of Reporters.
37	M	24	104.3	36	6	Yes	S	R	Ice was applied every four hours, and allowed to remain half an hour.	Charles A. Currie, Philadelphia.
38	F	38	102.5	.....	.....	...		R	Acute pulmonary congestion. In bed two days. Pain, pyrexia and dyspnea were promptly relieved.	Arthur Dedoe, Seattle, Wash.
39	M	69	102.5	.....	.....	.....	S	R	Acute congestion right apex. Complication of serious aortic incompetency. Cold was substituted for hot poultices, which gave no relief. Patient was out in three days.	" "
40	M	6	104	.....	.....	.....	S	R	Reporter's son. Cold gave prompt relief.	" "
41	M	15	104.4	.....	6	No	S	R	In hospital nine days. Good recovery.	J. C. Knipe, Jewish Hospital, Phila.
42	M	28	105.2	40	.....	Yes	S	D	Was in poor health with previous cough. Died on sixth day.	" "
43	M	38	102	.....	5	No	S	R	In hospital seven days.	" "
44	M	20	104.6	.....	4	No	S	R	In hospital eight days. Good recovery.	" "
45	M	20	104.8	.....	5	Yes	S	R		" "
46	M	33	103	32	7	Yes	S	R		" "
47	M	27	104.2	32	.....	Yes	S	R		" "
48	F	17	104.2	48	.....	Yes	S	R	Very grave case. Was poulticed with hot applications for five days after the chill, but without influence on fever, delirium or restlessness. After this period ice was applied and the gravity of all the bad symptoms was markedly and almost immediately abated.	David F. Monash, Des Moines, Ia.
49	M	23	103.4	40	5	No	S	R	Cold gave relief to pain, dyspnea and restlessness.	" "
50	M	34	104.4	38	5	No	S	R	On account of friends' objection ice was not applied until third day. Benefit apparent at once. Patient addicted to alcohol and opium. Dr. Monash adds: "I strongly urged the use of cold in two other cases in another family, but neither the friends nor the attending physician would agree to it. One died, and the other recovered from the acute process, but merged into tuberculosis."	" "
51	M	60	103.8	40	6	Yes	S	R	Patient himself recognized the comfort derived from ice-bag. "I consider the use of ice in acute pneumonia of as great value as antitoxin is in diphtheria."	W. J. Fletcroft, Paterson, N. J.
52	F	36	104	40	7	No	S	R		" "
53	M	16	104	40	5	No	S	R	This patient, although he made a good recovery, had another attack of pneumonia three months later. (See next case.)	" "
54	M	16	105.4	40	7	No	S	R	Until the fever had risen to 105° on the fifth day the patient was treated with hot poultices. After this ice was applied and the fever dropped at once.	" "
55	M	25	104.4	40	4	Yes	S	R	Ice was applied on the fourth day and in twelve hours crisis occurred. Patient had insomnia from time of chill until ice was applied. In his delirium he jumped out of second-story window. Recovery was slow.	" "
56	F	36	.....	.....	.....	Yes	S	R	Patient was not seen until four days after chill, when ice was applied. Good recovery.	Geo. S. Stone, New Freedom, Pa
57	M	10	.....	.....	.....	No	S	R		" "
58	F	18 mos.	.....	.....	.....	No	S	R	The last three cases were mother and two children, the attack in the first beginning on March 8, and that in the children on March 10, 1896. The two ended in crisis and the first in lysis.	" "
59	M	78	.....	.....	2	No	S	R	Pneumonia ran its course in forty-eight hours.	" "
60	M	22	.....	.....	.....	No	S	R	Patient improved from time ice was applied.	" "
61	M	40	.....	.....	.....	Yes	S	D	Patient addicted to alcoholic excess. In an intoxicated state, he lay a whole night on the frozen ground. Unconscious most of the time.	" "
62	M	20	.....	.....	4	No	S	R	Ice gave immediate relief.	" "
63	M	40	104	.....	.....	Yes	S	R	Patient alcoholic. Expectorated much blood.	" "
64	M	20	104.5	.....	.....	Yes	S	R		" "
65	F	38	105.5	.....	.....	Yes	D	R	Whole chest enveloped in ice. Patient begged for ice when it was removed. "Am confident the ice saved her life."	" "
66	M	8	106	.....	9	Yes	S	R	Temperature fell directly after ice was applied.	C. B. Bastian, Salladasburg, Pa.
67	M	6 mos.	106	.....	.....	.....	S	R	Pneumonia complicated with convulsions.	" "
68	M	12	104	.....	.....	Yes	S	R		" "
69	F	6	.....	.....	.....	Yes	S	R	Reporter believes ice limited the extent of inflammation.	" "
70	M	20	104	.....	.....	Yes	S	R	Reporter gave case up as hopeless. Made a slow recovery.	" "
71	M	10	105	.....	.....	Yes	S	R	Pain so severe that opiates were required.	" "
72	M	8	.....	.....	.....	No	S	R		" "
73	M	46	103	.....	.....	Yes	D	R	Different areas were successively invaded. In bed three weeks.	" "
74	M	10	104	.....	.....	Yes	S	R	"Ice always lowered the temperature."	" "
75	F	70	103	.....	.....	Yes	S	R	"Think ice saved this patient."	" "
76	F	6½	105	44	.....	No	S	R	Reporter states that he is a firm believer in the efficacy of the ice-treatment of pneumonia: and adds, "I regret very much that, owing to loss of my notes, I am unable to give the history of HALF A DOZEN other desperate cases (not tabulated) treated in the same way, all of which, except one, I think owe their lives to the cold treatment."	J. R. Care, Worcester, " " Pa. " "
77	F	27	104	36	.....	Yes	S	R		
78	M	16	105	36	.....	Yes	S	R		

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and addresses of Reporters.
79	M	28	103.5	28	3	No	S	R	This case began with great dyspnea and pain in the right lung. The ice relieved these symptoms.	B. F. Kierulff, Marshalltown, Ia.
80	M	9	105	30	3	Yes	D	R	Ice was also used internally with great benefit.	" "
81	F	40	104.2	50	8	Yes	D	R	This was a critical case of pneumonia in which an unfavorable prognosis had been given by the attending and consulting physicians; and while passing through the attack the patient gave birth to an eight-months' fetus. Her delirium was violent, sometimes alternating with muttering and subsultus tendinum, but after the ice application, all these grave symptoms gradually subsided. The ice was used as "a forlorn hope."	A. H. Boyer, Philadelphia.
82	M	50	105	50	10	Yes	S	R	This was a very aggravated case and had been abandoned by another physician. Reporter thinks the ice and strychnin saved him.	A. Chas. Dogge, Helena, Mont.
83	M	38	105.5	60	5	Yes	D	R	A very unfavorable case, suffering with chronic articular rheumatism and endocarditis.	" "
84	M	13	104	50	9	Yes	S	R	Dyspnea great; face cyanosed, but so soon as ice was applied improvement occurred.	" "
85	M	21	104	.....	.....	Yes	D	R	Ice of great benefit here.	" "
86	F	35	104	40	.....	Yes	S	R	Ice aggravated dyspnea, pain and restlessness. These symptoms were relieved by the administration of small doses of hydrogen dioxid.	" "
87	F	26	.....	.....	.....	Yes	S	R	Followed an attack of typhoid fever.	" "
88	M	46	103.5	.....	.....	Yes	S	R	At first objected to ice, but finally consented to its use. Result very good.	" "
89	M	40	105.5	50	.....	Yes	S	R		" "
90	M	20	104	45	.....	Yes	D	R		" "
91	M	35	.....	.....	.....	Yes	S	R	At the same time these cases were treated, reporter had another case of pneumonia in which, because he was not permitted to apply ice, he used hot poultices, and the patient died.	" "
92	F	25	105	.....	.....	Yes	D	R		" "
93	M	30	105	40	.....	Yes	S	R		" "
94	F	47	102	48	7	.....	S	R	Marked orthopnea and cyanosis. Twenty-four hours after ice was applied, the cyanosis had almost entirely disappeared and the patient was able to lie down and sleep; her respirations had fallen from 48 to 24, and her pulse-frequency from 110 to 90.	Charles A. Currie, Philadelphia

Sex.—The sex is noted in ninety-four cases, of which sixty-three are males and thirty-one are females.

Age.—The age is also given in ninety-four cases. The number occurring in each decade is as follows: Under one year, 1; between one and ten years, 22; between ten and twenty, 16; between twenty and thirty, 20; between thirty and forty, 16; between forty and fifty, 7; between fifty and sixty, 4; between sixty and seventy, 3; between seventy and seventy-five, 2; between seventy-five and eighty, 2.

Highest temperature-range.—This is given in seventy-six cases as follows: In two, 102°; in two, 102.5°; in six, 103° and 103.5° respectively; in twenty-four, 104°; in seven, 104.5°; in seventeen, 105°; in nine, 105.5°; in three, 106°.

Highest respiratory frequency.—This is noted in forty-seven cases. In one it was 28; in two, 30; in four, 32; in one, 35; in six, 36; in one, 38; in sixteen, 40; in three, 44; in one each 45, 52, and 80; in three, 60; in two, 48; and in four, 50.

Days of crisis or subsidence of fever.—This is noted in fifty-two cases. In four this occurred at the end of the second day; in seven, at the end of the third day; in seven, on the fourth day; in thirteen, on the fifth day; in seven, on the sixth day; in five, on the seventh day; in four, on the eighth day; in four, on the ninth; and in one on the tenth day.

Number of cases of single and double pneumonia.—There were seventy-six cases of single and fifteen of double pneumonia.

The mortality.—Out of the one hundred and four cases, three died—giving a mortality rate of 2.88 per cent. The ages of those who died were 28, 40, and 76 years respectively. Out of the total number of 299 cases so far collected there were ten deaths—giving a total death-rate of 3.35 per cent.

FEVER AND ACUTE PNEUMONIA IN THEIR RELATION TO COLD EXTERNALLY APPLIED.

As fever is such an important feature of pneumonia, and as I believe that a correct interpretation of the patho-

logic relationship between the two is indispensable in order to formulate principles for the correct treatment of both, I desire to take advantage of this opportunity to briefly discuss the general nature and pathology of fever, and afterwards, in connection with other neighboring lesions, to consider its special bearing on pneumonia from the therapeutic standpoint that is here advocated.

What then is the essential nature of fever? Does it make for health or for disease? Is it a process of evolution, or of dissolution? Does it build up or destroy? Is it an intrinsic evil, or is it a thing to be welcomed and encouraged within certain limitations in the hope that it will enable nature to fight and to overcome disease; or does it make the last state of the body worse than the first? The only rational answers that can be given to these questions are to be obtained through a study of the production of the normal temperature of the body, and this leads to the following questions: Where and how is the normal heat of the body produced and maintained? Where is the fire, and on what does the fire feed? Physiology teaches that the heat of the body is almost exclusively produced through oxidation, and that four-fifths of this amount comes from oxidation occurring in the muscles. In other words the muscles contain something that on being oxidized furnishes nearly all the heat of the body; or, a part of the body is burnt up for the purpose of supplying its own temperature.

Now, while heat is being thus generated there is at the same time another process in active operation, viz., that of heat-loss, or heat-dissipation. This serves as a heat-leveler and tends to limit heat-accumulation in the body. From an estimation of these two factors it is evident that the degree of temperature present in the body at any moment is dependent on how much is produced and how much is lost at that time; or, in other words, the degree of body-temperature at any time is the balance between heat-production and heat-dissipation. We are told that in the early days of railroad-engineering it was impossible to operate a locomotive in rainy weather on account of the cooling effect that the rain had on the steam-boiler. Dissipation of heat became too active here, and the locomotive was disabled in consequence. It is evident, therefore, that in order to preserve the function of the machinery in the human body or in the locomotive, neither heat-production nor heat-loss must go on without restraint; and that both must be adjusted in such a manner that no more heat is produced or lost than is necessary to carry out this purpose. This is exactly the case in health, for here the temperature is almost absolutely constant and uniform because it is under the control of a specialized nervous apparatus known as the thermotaxic mechanism, which regulates both heat-production and heat-dissipation. When the function of this mechanism becomes deranged, or is overthrown, either heat-production or heat-dissipation, or both, become disordered, and fever, or hyperpyrexia, or in some instances a subnormal temperature may supervene.

The history of the locomotive furnishes an appropriate prototype of the development of this thermotaxic apparatus. In the crude way in which the former was at first constructed no provision was made for preserving a normal relationship between heat-production and heat-loss; but by degrees this difficulty in construction was overcome, and in the machinery of the present locomotive there is established such an adjustment between heat-production and heat-loss that it may be operated under conditions that formerly made this impossible. In the lower forms of life, as in the case of molluscs and reptiles, the thermotaxic mechanism is but poorly developed, and the animal's temperature, like that of the early locomotive, is at the mercy of the surrounding temperature. The temperature of a warm-blooded animal poisoned with curare, like that of the cold-blooded animal, corresponds in rise and fall with the temperature exterior to the body—the influence of the thermotaxic mechanism is entirely incompetent to regulate the bodily temperature, because curare paralyzes the peripheral nerves. In the first days of extra-uterine life the temperature is subject to wide variations—showing that at this period of life the thermotaxic mechanism is excessively unstable. Thus it is plainly seen

that the development of the function of heat-coördination in animal life runs in a line parallel to that of the perfection of the steam locomotive. In the primitive history of both, heat-control is unsteady and defective, while in their higher stages of evolution this function has attained a great degree of stability and perfection.

It has already been stated that the nature of the thermotaxic mechanism is nervous and it is of interest to know that its anatomic seat is pretty clearly defined. According to Ott, Aronsohn and Sachs, Eulenberg, and Landois, there are six heat-regulating centers—two in the cortex, and four at the base of the brain. These experimenters have also shown that electric or mechanical irritation of these centers produces a rise of temperature lasting for hours at a time.

From all that has been said it is quite evident that Finkler is correct in describing *fever as a neurosis*, or more specifically, fever is due to disturbance of the thermotaxic mechanism and can in no sense be looked upon as a healthy reaction of the organism against the cause of disease. One might as well view the bending, the twisting, the creeking and the crackling of forest trees in a tornado as evidence of a normal reaction against the fury of the storm. In one sense it is a reaction of those that survive, but more correctly it is a contest that shakes and shatters and threatens the life of every tree that is attacked, and in which those that fail to possess the requisite resistance go down, while those that weather through it, perhaps, never fully recover from its effects.

Fever is always evidence that the function of heat-coördination is disorganized and has lost its power of restraining heat-production or heat-dissipation, or both, and is *per se* a morbid process. Fever may be compared to a heart whose beats run wild when certain cardiac nerves are irritated or disturbed. The heart, under these circumstances, does not beat with greater rapidity because there is greater resistance to be overcome in front, nor is its strength enhanced, but it behaves in this manner because the restraining influence of some important nerves is crippled. So, fever is a manifestation of depreciated nerve-integrity, and never serves the interests and purposes of health. Not only is fever an ordinary disease, but it has been shown by Vincent that the blood of animals that die from the effects of artificial fever contains a poison that causes convulsions, stupor and death, in guinea-pigs, sparrows and frogs.

From these brief considerations it is quite evident that fever is caused by disorder of the heat-centers—the principal ones of which are located at the base of the brain; and it is therefore of great interest to bear in mind the close anatomic relation of these centers to another important center which is located in the same region and which has a special affiliation with respiration. It is well known that a dog pants to cool himself off through the lungs, and that a fevered animal breathes rapidly in order to increase the rate of heat-discharge through the same channel. Richet, Ott, and other physiologists believe that the rapid breathing under these circumstances is due to the fact that fever-heat stimulates an independent nerve-center at the base of the brain which has the power of accelerating the respiration-rate, and which they call the thermo-polypneic center. Other observers hold that this phenemenon is brought about through the instrumentality of the respiratory center, which is stimulated to increased activity by the same cause, *viz.*, fever-heat. I believe that the evidence in favor of the existence of a special polypneic center is well made out; but whether it is or not, or whether the respiratory center, which is also located at the base of the brain, subserves the function of accelerated breathing during fever, it is clear enough that an increased respiration-frequency is always associated with acute pneumonia. So, too, there are also very good reasons for believing—reasons that I have discussed in my second collective report on acute pneumonia—that the process of pneumonia itself is in a great measure dependent on serious disturbance of the nervous system in which the respiratory nerves and center participate very largely, and hence the increased frequency of breathing in this disease may be due partly to central

and partly to reflex influences. Moreover, in close anatomic contact with the respiratory, the polypneic and the heat centers, is the convulsive center, and in this close companionship we have, perhaps, an explanation why almost all inflammatory diseases that implicate the base of the brain are attended with a frequent respiratory rate, high fever from the onset with little fluctuation, together with convulsions occurring with greater or lesser frequency. Pneumonia and cerebro-spinal-meningitis are prominent examples of such affections, especially when they occur in the infantile period of life. The reason why this combination of symptoms is developed more frequently in the young than in the old is because as age advances the various nerve-centers become less impressionable and more independent of each other, and hence are less easily thrown out of equilibrium by incidental disturbances. Then, beside the three kinds of centers already spoken of, the oblongata contains another one, *viz:* the cardiac center, which is invariably involved in pneumonia, and plays a very prominent *rôle* throughout the course of this disease.

In thus analyzing some of the important pathologic factors in the nervous mechanism of acute pneumonia the therapeutic indications in this disease are brought out very prominently. These resolve themselves into (1) an abatement of the local process in the lungs; (2) a reduction of fever, and (3) an alleviation of cerebral and general nervous irritation. Now, how are these extensive indications to be met? What is the remedy, or does it require more than one? After due consideration of all the clinical and experimental data so far as I know them, I believe that cold applied externally will meet these demands more satisfactorily than any other measure. Not only will it allay the morbid process in the lungs, but it possesses a direct ameliorating influence on those nerve-centers which, as we have seen, play a responsible part in the pathologic history of acute pneumonia.

EFFECTS OF COLD APPLICATIONS TO THE CHEST.

Cold, when applied to the chest, has a two-fold effect. First, it limits and checks the extension of the pneumonic process in the lungs and assists in bringing about resolution. This is borne out by the observation of any practitioner who has carefully watched the stoppage and the recession of this process as indicated by the modification of the physical signs. Secondly, through its action on the respiratory and the peripheral nerves of the thorax, cold has a profound sedative reflex effect on the heart, the respiratory and the polypneic centers—affecting these to almost the same degree as if it were brought in direct contact with them. Experiment confirms this by showing that when in an artificially fevered animal a piece of ice is applied directly to the polypneic and heat centers, as has been done by Ott, the respiration falls in frequency and the fever is promply reduced. Clinical experience demonstrates that this is precisely what happens when cold is applied to the chest in this disease. The respiration and pulse are reduced in frequency; the fever abates, sometimes falling several degrees in a very short time; the day of crisis is hastened, and if the attack is not aborted, its duration is materially shortened.

COLD APPLICATIONS TO THE HEAD.

Cold applied to the head acts directly on the brain and on the implicated centers at the base of the brain. Stress should be laid on the fact that such applications are always to be made in cases with high fever, great restlessness and general irritability, and we should, at the same time, remember that they are not out of place in less severe cases. They materially assist and reinforce the effects of the cold that is simultaneously applied to the chest.

METHOD OF APPLYING COLD.

The important practical questions that confront us are: How is cold to be applied and how long is it to be continued? In my practiee it is employed in the form of pieces of ice placed in rubber ice-bags. Others use snow, either in rubber bags or wrapped up in towels; or apply towels wrung out of cold water. The bags may be wrapped in towels, and, after they are applied over the inflamed area, a broad, thin bandage may be placed around the chest in order to keep the bags in place. This is of

special importance when the patient is restless and tosses about the bed. If the disease is confined to the front base on one side,only one good-sized bag will be necessary; but if the exudation extends higher up, or on the side or on the back, then one or two more bags must be applied laterally and as far back as possible. If the affection is extensive, as many ice-bags as are necessary to cover the whole area must be put on. I have had as many as eight ice-bags on the chest at one time. If the morbid process migrates, as it frequently does, it should be followed up with the ice and covered.

The length of time for which cold is to be used must, in most cases, be largely determined by the amount of fever present. If the temperature falls to or near the normal point, and shows a tendency to remain there, then the ice may be gradually removed. It is best, however, not to be in too much haste in withdrawing the cold, for frequently, when this is done, the temperature soon flies up again suddenly, and under these circumstances it seems to be more difficult to bring the temperature down than it was the first time. Sometimes, too, the temperature rises suddenly after the ice has been taken off, not because the disease has become active again in the old area, but because it has extended to a contiguous field of lung-tissue. It must be borne in mind, however, that the ice is not employed solely for the purpose of reducing fever, but also with the object of circumventing the exudative process and of hastening resolution. There may be very little fever present in some cases of pneumonia, yet the destructive changes are going on in the lungs at a rapid rate. Hence in senile, latent, or alcoholic pneumonia the activity with which the ice is employed must be governed by the impression that is made on the pulmonary disintegration.

Fever in pneumonia must, therefore, be regarded as an entity of powerful import, and one that demands special treatment. It is true that fever is not as simple and as gross a phenomenon as that which obtains in the local lung-lesion; yet it must be remembered that the latter is not the fundamental lesion from which the patient is suffering, but is merely the manifestation of some deeper or less apparent disorder. Just as pain, which is evidence of an irritated sensory nerve-center, is amenable to the influence of morphin, so is fever, which is the open and visible sign of some profound perturbation of those centers which coördinate bodily temperature, also to be abated and alleviated by the local application of cold.

A Quiet Wedding.

"Did you attend the Bircher-Jacklong wedding out at Billy-bee Dam last night, Ike?" asked the able editor of the Hawville *Clarion*.

"Aw, yes!" answered Alkali Ike, wearily; "I was thar."

"Of course there was a lively time, and all that?"

"Accordin' to how you looked at it. *I* called it mighty slow. Outside of the regular routine and the eatin', nuthin' happened worth mentionin'. To be sure, the preacher and the groom got into a row because the divine wanted his pay in advance; but that is a common occurrence. The Rev. Mr. Harps is too old a bird to take any chances. Of course Jack Howcome, the fiddler, had had too much, as usual, and this time he fell off from the table and broke his bow arm; Jack always was more trouble than he was worth. The bride's mother, who didn't like the groom anyhow, jumped on to the poor feller's neck jest before the ceremony with a rollin'-pin, and it took half a dozen of us ten minutes' hard work to tie the old lady fast in a rockin'-chair and carry her a few hundred yards out on the prairie. Durin' the rumpus somebody stole the feed, and we never saw hide nor hair of it all the rest of the evenin'. They accused me of it, but I was as innercent as a lamb. I hain't that kind of a man; and besides, I had a private snack of my own. That was every blamed thing that happened, except that a deputy sheriff slid in an arrested Coyote Pete for horse-stealin' or a little suthin' that a-way, and some feller had a fit. Shucks! Weddin's hain't noways what they used to be a few years ago. Lord! Them was the times! But now everything is gittin' to be too much like it is in the East."—From the "Editor's Drawer," in *Harper's Magazine*.

A CASE OF CEREBRO-SPINAL SYPHILIS.

AUGUSTUS A. ESHNER, M.D.,* PHILADELPHIA.

E. McG., a well-digger, thirty-five years old, was referred by Dr. W. Campbell Posey from the Eye Department to the Department of Nervous and Mental Diseases at Howard Hospital, with the report that both visual fields were restricted, in conjunction with hemianopsia of the nasal halves, greater on the right side. The discs were atrophic, the walls of the retinal vessels thickened, the superior temporal artery being reduced to the size of a mere thread. The retina was thickened and hazy and there were evidences of an old chorio-retinitis. The pupils were unequal and sluggish.

The patient complained of impairment of vision with the right eye, which had been noticed for four or five months and had been gradual in onset and invasion and progressive in course. There had been subjectively no difficulty with the left eye.

On inquiry the patient related that he had had a paralytic stroke nearly a year before, which occurred while he was at work. The right upper extremity became powerless and during the night the left lower extremity was affected similarly. Speech was at the time interfered with and consciousness was lost for two days. There was no convulsion, but the sphincters failed to act normally for six or eight days. Subsequently the patient improved, but he never recovered entirely. In the course of five or six weeks, after immoderate indulgence in alcohol, left-sided weakness set in, without loss of consciousness. The sphincters now remained active and the patient soon went about as usual.

On examination the gait was found to be decrepit and station unsteady. The knee-jerks were exaggerated and feeble ankle-clonus could be elicited on the right. The right pupil was larger than the left and eccentric. Both reacted to light. On extending the fingers of the right hand the second phalanges of the index and ring fingers were over-extended, but this condition, the patient stated, had existed from birth. There was no gross impairment of sensibility; no headache; no diplopia; no difficulty in micturition. Sleep was not disturbed. Vertigo was present at times. The patient admitted having had two venereal sores on the penis seven years previously and subsequently two sores on the left leg. Three children were healthy and there was no history of miscarriage. One child died a day after birth.

Upon treatment with potassium iodid in saturated solution, rapidly increased in dose to fifty drops three times a day, progressive improvement took place.

This case does not require extended comment. It bears the unmistakable stamp of syphilis of the nervous system. Apart from the history of venereal infection and improvement upon treatment with iodids, the wide and irregular distribution of the symptoms, without definite interrelation, must be referred to a group of lesions, such as for practical purposes only syphilis causes. Of course, other disorders of multiple seat, as for instance neoplasms or tuberculosis, may give rise to similar manifestations, but there are usually other symptoms that make clear the real nature of the case. For the privilege of reporting this case I am indebted to Dr. Lewis Brinton, in whose clinical service I had the opportunity of observing it.

*Professor of Clinical Medicine in the Philadelphia Polyclinic; Physician to the Philadelphia Hospital, etc.

Dilettante (very pressing)—I should like so much to write for your newspaper. One side of the paper has to be blank, hasn't it?

Editor—No; both!—*Fliegende Blatter.*

He—Who is that man over there?

She—Professor Smart, the great mind reader.

He—Ah, I suppose he mingles with society when he wants to have a rest.—*N. Y. Herald.*

PURULENT PERITONITIS WITH STRICTURE OF THE RECTUM.*

HENRY J. WALCOTT, JR., M.D.,† PHILADELPHIA.

Annie S——, thirty-eight years old, married, house-wife, was admitted to the ward November 4, 1896, with the following history:

Family history negative, had ordinary diseases of childhood, enjoyed good health up to the age of twenty-three when she contracted syphilis. From that time up to the date of admission her health has been poor, her principal trouble being obstinate constipation. This began twelve years ago and has become progressively worse. Five years ago she passed large amounts of pus and blood by the bowel from time to time until one year ago last July when, as she expressed it, "some sort of an operation was done" at one of the hospitals in this city. This operation, whatever it was, gave some relief, but gradually the constipation became more and more marked, the patient going at times so long as two weeks without a movement. She was unable to take an enema as it caused great pain, and medicine had to be taken in large and increasing quantity by the mouth to secure a bowel movement.

The present attack dates from 4 a.m. on the day of admission. While nursing a sick child of a neighbor she suddenly complained of great pain in the right iliac region, of vertigo and faintness. Her bowels not having moved for four days, the pain was attributed to that fact. The pain becoming more severe a physician was called in and he promptly had her removed to the hospital in the ambulance.

On admission (4 p.m., November 4,) her temperature was 101°, pulse 138, respirations 16. She complained of severe pain, especially upon the right side. Her abdomen was somewhat distended, rigid, very tender on pressure and her expression was one of anxiety and fear. Her condition becoming worse, despite all measures used to relieve her, I sent for Dr. Montgomery and at nine o'clock in the evening the patient was given ethyl bromid and an examination made per rectum.

About four inches from the anus a stricture was encountered, which narrowed the calibre of the rectum to such an extent as to hardly permit the introduction of a small finger. A large mass of hardened fecal matter lay above this. The stricture was dilated by means of the fingers and a dose of castor oil (f℥iss), given in the hope of softening the fecal mass, but without success. A large enema was also carefully given but was not retained. The temperature at midnight was 100°, pulse 136°, respirations 36. The pain became so severe that a hypodermatic injection of a quarter of a grain of morphia sulphate was given into the wall of the abdomen. This relieved the pain to a great extent. Ice-bags were applied and one-twentieth grain of strychnia sulphate was given hypodermatically every three hours.

Soon after admission the patient began to vomit, the vomited matter consisting of a dark brown liquid without fecal odor. The usual measures failing to check the emesis, lavage was performed, boracic acid solution to the amount of two quarts being used. This had the immediate effect of stopping the vomiting and the patient became proportionately more comfortable.

At 4 a.m., November 5, her temperature was 101⅘°, pulse 140, respirations 32. At twelve o'clock noon the temperature was 101⅘°, pulse 170, respirations 36. During this time we had been unable to empty the bowel by means of magnesia sulphate, magnesia citrate, castor oil, enemas, etc. The patient became weaker and weaker, and, in spite of all measures instituted for her relief, died twenty-four hours after admission. Her condition from the time of admission to the time of her death was such that all operative measures were deemed inadvisable and the wisdom of such a decision was borne out by the autopsy.

Urinary analysis.—Amber. Acid. Sp. Gr., 1,026. No sugar. Trace of albumin,

* Service of Dr. E. E. Montgomery.

† Resident-Surgeon St. Joseph's Hospital.

epithelial, granular and tube casts, urates and mucus.

Post mortem.—Upon opening the abdominal cavity, a stream of thick, greenish, foul-smelling pus flowed out. It was not possible to measure it, but certainly there was not less than a quart. Numerous adhesions were present. The thoracic cavity contained a small amount of bloody fluid. The right lung was adherent throughout its entire extent, the left lung slightly adherent. The pericardium was also adherent. Most of these adhesions were of long standing but those in the abdomen were of more recent formation. The pericardium was thickened and contained an excess of fluid. Heart, normal in size and consistency. The right ventricle contained a large "currant jelly" clot. The right auricle and left ventricle were empty, and the left auricle contained a small amount of blood. The valves were normal.

The lungs were normal in size, but the right was congested, left being normal. Spleen slightly enlarged. Kidneys, those of a chronic parenchymatous nephritis. Ureters normal; bladder wall much thickened. Uterus normal, cyst of tube on left side about the size of a hen's egg; right ovary and tube normal. The stomach was slightly dilated and contained a portion of solution used for lavage; mucous membrane congested. The gall duct was obstructed; liver normal in size and color, adherent to diaphragm for its entire extent. The gall bladder was distended, projecting one-half inch beyond margin of liver and contained blood.

The small intestine was normal in size but in the ileum there were evidences of old ulcerations. These were three in number and consisted of little cup-like depressions each of which contained a fecal concretion. The large intestine was contracted throughout its entire length, but the most marked contraction was in the eighteen inches of the gut nearest the anus. This portion was so contracted that the introduction of the little finger (a trifle larger than a lead pencil) was impossible. Appendix vermiformis, normal. No examination of the brain was made.

The autopsy showed that operation would undoubtedly have proved futile, for, had it been possible to check the violent peritonitis already set up, the chances for a good result in intestinal resection would have been comparatively *nil*, owing to the large extent of gut involved.

HAY ASTHMA.*

J. NEWTON HUNSBERGER, M.D., SKIPPACK, PA.

The definitions of this disease are to a certain extent unsatisfactory. Some writers describe it as a form of catarrh; while Osler defines it as an affection of the upper air passages, often associated with asthmatic attacks, due to the action of certain stimuli upon a hypersensitive mucous membrane. It is evident that the neurosis may be brought on by a number of irritating substances. I say a number, for at various times between seventy-five and one hundred different substances have been supposed to be the cause of hay fever. Of course among such a great number it is hard to say just which irritant is the cause of the trouble. Our friends interested in bacteria must have overlooked this interesting affection or they would surely have given us, from their overstocked laboratories, a few little innocent microörganisms on which to fasten the blame. After irritants, seasons and heat are also exciting causes.

It is true that attacks may come on in certain individuals at any time during the year, yet the greater number are liable to attacks between the months of May and September. We may have two forms—"rose cold" in May and June, and "hay fever" in August and September and until frost. Of course the heat of the summer months and the

*Read before the Montgomery County Medical Society.

presence of irritants in the air, as inorganic dust of all kinds and the pollen of plant life, all assist in precipitating attacks of the neurosis. Overwork, worry and the resulting indigestion, or, in fact, anything which tends to lower nervous tones may almost be considered exciting causes, while they surely predispose to attacks. It is not so much too much heat and sunlight as the depressing effects which follow, that predisposes some individuals to this annoying affection.

Locality certainly is a factor, but I do not believe it can be classed as an exciting cause. I believe that the views held by some writers will be greatly modified when the subject of locality is more thoroughly studied, with hay fever considered, not as a true pathologic condition, but as a neurosis.

Hay fever is a neurosis pure and simple. Of course any chronic nasal catarrh or a pathologic change or growth in the air passages would tend to aggravate the condition. In a case under my care I removed a polypoid growth from the nares with an immediate amelioration of the symptoms, but with no further effect on the course of the disease.

It is hard to say whether the nerve filaments of the nasal passages are affected, or whether the centers directly or indirectly controlling these filaments are the cause of the disease. But I believe the sympathetic system has a great deal to do with the trouble. There is great vaso-motor disturbance, general as well as local. The slight chilliness, mild febrile condition, depression, flashes of heat, together with a disturbance of the organs of indigestion, all point to a change in vaso-motor balance. The local symptoms are so well known as not to need repeating, but are not truly an inflammatory condition, but simply a vaso-motor dilatation of the capillaries of the air passages.

To prove this we might point to its freedom from true pathologic conditions, its dependence on external irritants, to which a vastly greater number of persons are exposed than are affected; its close connection with allied nervous conditions, as false croup, certain forms of asthma, etc.; its annual periodicity, the subjects being perfectly well and strong, and showing no unusual susceptibility to ordinary colds in the intervals; the sameness of the symptoms in different individuals, while there is the greatest difference in the special exciting causes; and the rapid and complete cure effected by a change of locality, often even if this change is but a matter of a few miles.

MacKenzie reports a case of "rose cold" brought on by the odor (?) of an artificial rose. And the mere fact that there is in existence a United States Hay Fever Association, to which only those who can afford an attack of hay fever are eligible to membership, shows that the pleasures of the cure counterbalance some of the annoying symptoms.

To prove further the non-inflammatory character of the disease I will cite a case reported by Dr. Geddings: "While residing at Bethlehem, N. H., I was called one evening to see a German who had just arrived on the train from Fall River. His condition was most pitiable; his eyes were fiery red, the nose and face were terribly swollen, while the water streamed from both eyes and nose. The asthma was at its height, and his struggles for breath were fearful in the extreme. I injected a quarter of a grain of morphine into his arm and left him for the night. The next morning, while preparing to pay him an early visit, the patient himself appeared at my office, bright and cheerful, and so much changed that I at first failed to recognize him. A single night had served to dissipate all traces of his hay fever."

Some years ago a girl about thirteen years old came to my office with a periodic attack of hay fever. I prescribed for her, and heard nothing from her for several weeks, when I was informed my prescription did her no good but that Dr. Blank had cured her with one bottle of medicine. I inquired of the doctor what the wonderful remedy was and was informed that paupris and nitre effected the cure. She has had two attacks since then but the paupris and nitre have not cured them, neither has any other drug.

The treatment is simple to non-members of the Hay Fever Association. A little bromide and three to six weeks' time always effects a perfect cure.

COMMUNICATIONS.

ANTI-DIPHTHERITIC AND ANTI-STREPTOCOCCIC SERUMS–THEIR NATURE, METHOD OF PRODUCTION, AND APPLICATION FOR THE RELIEF OF DISEASE.*

C. C. FITE, M.D., NEW YORK.

Those of us who received our medical education long enough ago to be fond of retrospection will recall the ready acceptance which was given to the germ theory of disease, by the younger and more plastic minds in the scientific world when it was promulgated. We can also recall the doubt and ridicule that was heaped on the promoters of this theory by the over-wise and the ultra-conservative element.

It was my good fortune to witness Lister operate, and I noticed how his spray and apparatus excited the ridicule of some of those over-wise men who stood near; men who, by the way, have been forgotten, whilst Lister's name will live forever. Listerism, Pasteurism, antisepsis, asepsis, whatever name we may give to the methods then or since used, mean, after all, the same thing, fighting noxious germs, killing the microbe that is, either by its own destructive action or through poisonous secretions, destroying humanity. All we have attained to-day in serum therapy comes legitimately from the establishment of the germ theory of disease.

This paper will be limited to the question of the proper method of producing and using substances to either kill or neutralize the effect of disease germs. The anti-diphtheritic serum has been accepted as a part of our legitimate and definite list of remedial agents. The anti-streptococcic serum is still on trial.

The theory of the nature of anti-diphtheritic serum is a simple one. The diphtheritic germ, the Klebs-Loeffler bacillus, finds lodgement in the throat or elsewhere. It begins to grow and multiply, a membrane is formed, a poison is secreted and absorbed. This toxin overwhelms the life centers. Paralysis and death may follow. Nature has in the meantime been endeavoring to overcome the danger that is threatening the patient, and has secreted an antitoxin to destroy the toxin.

Nature gives the germ a life, but when the balance is apt to be against the culture field, the body, endeavors to neutralize the danger in her own mysterious way. This process was repeated millions of times for thousands of years, but we did not understand it, did not go deep enough. Finally the educational results of the germ theory had produced a long list of careful thinkers, and with the microscope, and by the aid of vivisection on animals, the secret was discovered, and now, all over the civilized world, are laboratories for the production of the fluid.

The method of manufacturing anti-diphtheritic serum is in brief as follows: First a culture medium is prepared by adding bouillon to blood serum, and then coagulating the mixture; this is known as Loeffler's blood serum. The diphtheria germs are secured by passing a sterile swab over the false membrane and the swab is then passed over the blood serum and the tube containing the now infected serum is closed with a cotton plug, and put into an incubator where it can be kept at the body temperature. After a few hours small colonies appear on the surface of the serum and the microscope is used to determine if the Klebs-Loeffler bacillus is present, and if so, if it is free from other forms of life. If it is a pure culture, small colonies are picked out and transferred to fresh serum and replaced in the incubator.

*Read before the Lycoming County Medical Society, Williamsport, Pa., December 1, 1896.

These growing germs are the seed used for producing the toxin; they are planted in sterilized beef bouillon. The germs then grow rapidly, and produce or secrete toxins which are retained in the bouillon. A small amount of a preservative is added and it is then filtered through unglazed porcelain, which removes the germs and all foreign bodies, leaving the solution clear. This solution of toxin is then injected into the jugular vein of a horse.

The horse's blood now contains an antitoxin which destroys the toxin we have been putting in it. The dose is repeated until, in the course of a few months, a horse can stand without injury a dose several hundred times stronger than one that would have killed him at the beginning of the treatment. The horse is then our laboratory, but Nature has yet kept her secret as to how the antitoxin is produced. All we have to do now is to secure the blood, remove the clot, filter, sterilize and preserve the serum, and we have the result, a package of anti-diphtheritic serum, and we are ready to go on our life-saving errand.

I have purposely avoided any reference to the various methods and tests applied on guinea pigs and with the microscope to determine the strength of the toxins and antitoxins from time to time. The methods have been carefully worked out by those engaged actively in the work, and in this way they have been enabled to establish a fixed standard just as definitely as we could weigh any given drug with the scales. The method now generally in use is to make a given number of antitoxin units a dose, irrespective of the bulk.

It is of the utmost importance that the serum should be so highly concentrated as possible, hermetically sealed, and only one dose in a package, and it should not be opened until ready to use it. By being careful about this, and using a clean syringe, avoiding the large old-style antitoxin syringe with the rubber tube, the physician can be confident that no harm will come from its use. My opportunities for observing the use of anti-diphtheritic serum and for conferring with eminent authorities as to its use have been quite extensive, and I have reached the conclusion above given and also that it is of the utmost importance to use the serum as early as possible and in full and repeated doses. I think it is legitimate to add that the majority of those who have not been successful in their use of antitoxin have either not used it in sufficient quantity or early enough. Dr. W. A. Walker (*Pediatrics*, October 15, 1896), places great stress on this point, as also does Dr. Douglas H. Stewart (*Annals of Gynecology and Pediatrics*, November, 1896).

Allow me to make a brief digression here and refer to the value of using the microscope in making a diagnosis. Dr. William Osler kindly showed me a case of typhoid fever in his service at the Johns Hopkins Hospital which had a small patch of false membrane on the lower lip. Microscopical examination showed the Klebs-Loeffler bacilli in abundance. It is often doubtless true that the real nature of cases of this kind are overlooked, and they become the foci of infection, and the patch, even though small, may spread to other surfaces later on, after having been the means of infecting probably a number of people. Then we are sure to hear the cry raised about sporadic cases!

Do not neglect other methods of local and general treatment; sustain the patient's strength, keep the bowels open with calomel and keep the throat clean. The Loeffler solution is an admirable combination of antiseptics for local use.

Marmorek and others have used anti-streptococcic serum in cases of diphtheria, showing the streptococcus as well as the Klebs-Loeffler bacillus, also in scarlet fever, puerperal fever, general septicemias, infective tonsillitis, erysipelas, and other diseases, whether due to, or complicated by, the appearance of the streptococcus pyogenes, and it seems as if it will prove of great value in other cases, perhaps in multiple abscess and in broncho-pneumonia, or in fact wherever the streptococcus is found. Dr. Henry Dwight Chapin has been giving some attention to its use in the two diseases last named.

In reference to its use in diphtheria, I am firmly convinced that it is indicated in all cases where the microscope shows the streptococcus and we almost invariably find it in cases which do not yield to the anti-diphtheritic serum and

where we see a zone of inflammatory action extending beyond the area occupied by the true diphtheritic membrane. Therefore, if a case does not yield promptly to the anti-diphtheritic serum, we should, I believe, use the anti-streptococcic serum without waiting for the report from the bacteriologist.

The method of producing the anti-streptococcic serum is in the main a similar process to that used in the production of the anti-diphtheritic serum, or by injecting virulent cultures of the germs instead of the toxin.

Dr. Charles T. McClintock of Ann Arbor writes as follows: "As regards the difference in methods for producing anti-diphtheritic and anti-streptococcic serum, I may say in general that the streptococcus, like a number of other germs, does not readily give off its toxin to the surrounding liquid. If you want to get the toxic properties of the germ, you must take its own protoplasm. On this account we are compelled to use the germs themselves, in order to successfully immunize an animal. The filtered toxins are not very powerful. This is the essential difference between the two materials. In the case of the streptococcus we use a living virulent germ in bouillon culture."

Dr. Dillon Brown of New York, says: "To test the value of a streptococcus antitoxin we must not use the toxin; but the culture itself. The point is that in streptococcus infection, the germ itself finds its way into the blood and viscera, while in Klebs-Loeffler infection, the bacillus is rarely found in the viscera on autopsy. In one case, you have a toxin only to fight, and in the other, you have both the germ and its toxin."

There has been a great deal of good work done quite recently in this investigation of the anti-streptococcic serum and I feel confident that we are near a solution of the problem. The New York Board of Health is having it carefully studied in the Willard Parker Hospital.

The great lesson we may learn from the history of serum therapy, bearing upon scientific advancement, is that we should keep our minds open to receive testimony and look forward, not backward. We are at the beginning only of a great era in medicine. Medicine is partly a science, partly an art. The laboratory worker and the student are scientific producers of the colors which the practitioner, the artist, puts on the canvas. If we work together, the one investigating and producing, the other investigating and using, we make more rapid strides, get quicker results, and mutual confidence produces mutual good-will and a cheerful exchange of ideas of advantage to all.

A certain doctor had occasion, when only a beginner in the medical profession, to attend a trial as a witness. Counsel, in cross-examining the young M.D., made several sarcastic remarks, doubting the ability of so young a man to understand his business. "Do you know the symptoms of concussion of the brain?" asked the learned counsel. "I do," replied the doctor. "Well," continued the attorney, "suppose my learned friend, Mr. Bagwig, and myself were to bang our heads together, should we get concussion of the brain?" "Your learned friend, Mr. Bagwig, might," said the doctor quietly.—*Argonaut.*

It is estimated in Madrid that since the beginning of the Cuban revolution, the army of occupation has lost nearly fifty thousand men, of whom by far the greater number died from disease. The total number of patients in the military hospitals in one day recently was 9,475, of whom 1,035 were suffering from yellow fever, 1,331 from malaria, and 520 from wounds.—*Medical Record.*

A Quick Diagnosis.

At a surgical clinic a few days ago, before a class in the Harvard Medical School, a patient was shown who had a wound on the thigh caused by the bite of a rat. The instructor having asked the class for a diagnosis of the case, one of the students replied promptly, "Rodent Ulcer."—*Boston Medical and Surgical Reporter.*

"Oh, mamma," said little Willie, as he made his first close inspection of a bicycle, "this machine has got rubbers on to keep its wheels from getting wet."—*Exchange.*

THE MEDICAL AND SURGICAL REPORTER

ISSUED EVERY SATURDAY

THE BUTLER PUBLISHING COMPANY, Publishers

Editor and Manager
HAROLD H. KYNETT, A.M., M.D.

Associate Business Manager
WM. H. BURR, M.D.

Editorial and Publication Offices, 1026 Arch Street, Philadelphia, Pa.
P. O. BOX 843

Editorial Staff:
A. L. BENEDICT, A.M., M.D.
SAMUEL M. WILSON, M.D.

TERMS:—One year, three dollars in advance. Four months' trial, one dollar in advance. Subscriptions may begin at any date.
REMITTANCES should be made by Draft, Money Order or Registered Letter, payable to the order of the Butler Publishing Company.
COMMUNICATIONS for the Editor, and books for review, should be addressed to the Managing Editor of the MEDICAL AND SURGICAL REPORTER, 1026 Arch Street, Philadelphia.
BUSINESS COMMUNICATIONS, and letters referring to the publication, subscription or advertising department, should be addressed to the General Manager, P. O. Box 843, or 1026 Arch Street, Philadelphia.
CONTRIBUTIONS of value to the medical profession are invited from all sources. Original articles, contributed exclusively to the MEDICAL AND SURGICAL REPORTER, will be paid for at liberal rates, after publication (payments made quarterly), or a limited number of reprints will be furnished as the author may elect. Extra reprints will be supplied at cost rates to the journal. Orders for reprints *must accompany* MSS. To ensure the return of contributions not made use of, writers must enclose return postage.
THE MEDICAL AND SURGICAL REPORTER will not be responsible for the opinions of its contributors.

PHILADELPHIA, SATURDAY, DECEMBER 19, 1896.

PUBLISHERS' NOTICE.

Subscriptions to the REPORTER are payable in advance, and the season being at hand when renewals are most frequent, the publishers request each subscriber to examine the address printed upon the wrapper in which the journal is mailed. The date printed with the address is that TO which the subscription is paid. The subscriber will see at a glance the status of his account. Formal acknowledgment is made of every remittance as received, and the date upon the wrapper is revised accordingly. If not properly altered within three weeks, subscribers are requested to notify us of the oversight in order to prevent subsequent mistakes. Delinquent subscribers will learn the date since which their accounts are in arrears.

EDITORIAL.

AN EDITOR ON EDITORIALS.

The readers of the REPORTER may have noticed that, during the present year, most of the editorials have dealt with educational, ethical and other general subjects which are, in a sense, lacking in a practical bearing on medical art. It is questionable whether we may speak of this limitation of editorial discussion as the policy of the REPORTER, since it has arisen spontaneously and without a formal declaration of opinion by the editorial staff. Yet the fact that the members of the staff have each followed the same general course, shows a

unanimity of opinion much better than if the individual writers had been controlled by an explicit rule.

Many journals have followed the custom of basing editorials on reports of foreign medical progress. The subscribers to the REPORTER may have missed such editorials but they can not complain that the journal has ignored this subject, since it has been fully presented in a special department of the REPORTER, which has occupied a place in most of its weekly issues. We believe that a translation should appear as such, properly credited to its foreign source and with the name of the translator as a guaranty of correct rendering into English. The use of the writings of scientific investigators, diluted with editorial comment, does not seem to us a wise or proper custom, unless the matter in hand is of so great importance and involves so radical a change of medical opinion as to warrant its inclusion with the department of news.

Neither do we believe that original, scientific or practical articles should appear under the form of editorials. Such a method can readily be pardoned in a journal conducted solely by one man and having a limited circulation, for, in that case, every one knows exactly who is meant by "we" just as "he" stands for the husband and father in a family. But in a journal of wide circulation, reaching many readers beyond its immediate circle of subscribers and penetrating into distant States and countries, the editorial pages are not the proper place for original articles. In the first place, every writer and investigator has the right to whatever credit may attach to his work; in the second place, the reader of an article has a right to know—not merely surmise—by whom it is written. The editorial staff of a medical magazine may properly assume a more intimate acquaintance with medical news than the physician not connected with a journal; if they do not possess this superior knowledge, they have no qualification for the position which they occupy. They may also, without affectation, claim to be in closer touch with the ethical and educational needs of the profession than any one who has not given such matters especial consideration. But here, the authority of the editor stops.

When it comes to the practical consideration of medical topics and the preparation of articles dealing with diagnosis and treatment, the editor as such, is no more qualified to express an opinion than any other practitioner, although he may, from the necessity of keeping watch over advances in medical art, be better qualified to judge between various proposed methods, than an equally good physician who has not followed so closely the current history of medicine. Thus, in our opinion, the "practical" editorial should be limited quite closely to the balancing of rival methods, with the attempt, not so much to win adherents to one theory or another, but rather to allow an impartial consideration of the claims of each.

It is doubtless true that most editors of medical magazines have achieved at least moderate success in the practice either of general medicine or of some specialty, though some are purely literary men and incapable of practicing medicine except on paper. But the editorial general practitioner can scarcely pretend to instruct the specialist, nor can the editorial specialist lay claim to accurate knowledge of provinces of medicine in which he has no daily occupation. In short, so far as ordinary medical practice and opinion are concerned, he should drop the editorial mantle and speak from the authority of his individual information.

It is amusing and pitiful to note the didactic tone of the editors of a certain class of medical periodicals and the humble appeals for information of the correspondents. Such a reciprocity indicates a poorly educated clientele and an editor who is either pretending to information which he does not possess or who is giving elementary instruction which his subscribers ought to have had before they were turned loose on a suffering community. At the same time, we recognize fully the advantages of a friendly relation between editor and subscriber and are always glad to furnish such information as we may or to obtain it from other sources when our own fallibility is in evidence. But we believe that, as a rule, this function of the editor should be performed privately. Information as to illegal practitioners and violations of the law or spirit of proper practice, criticism and suggestions, by which the REPORTER may be made more useful, are always welcome.

We keep constantly before us the ambition to make the REPORTER the best practical medical journal of the country, to publish the results of the best study and observation—remembering that it is the contents of an article and not the name of its writer that must be considered—to present medical news and reports of the progress of the medical world in their proper departments and to confine the editorial expressions to matters on which the REPORTER may modestly hope to mould opinions and elevate the tone of the profession.

CORRESPONDENCE.

AN UNUSUAL IDIOSYNCRASY.

EDITOR OF THE REPORTER:—We not unfrequently meet patients who cannot take potassium iodid to the usual amount, without causing iodism. But, so far as I know, the following case stands by itself. It seemed necessary to get the man quickly under the influence of the iodid; and this prescription was written:

℞

Potass. Iodidi . . . . . ʒij.
Aquæ Pura . . . . . q. s. ad fl ℥ij.
M. Ft. sol.

Sig.—Take a dose well diluted in water three times a day. Take five drops the first dose and increase one drop each dose afterward.

When the dose reached twelve drops the patient said it made him feel so ill in the stomach, that he did not think he could bear a larger dose.

It was found that he was taking it before meals, in a teaspoonful of water, and he was told to dilute it in a half or one wineglassful of water, and take it after meals. Fifteen drops caused vomiting, and so he was directed to take it in milk, and to take a few drops of peppermint essence before taking it and to rinse the mouth with the essence after each dose.

When twenty-two drops was reached, he reported that he had a couple of "nervous spells" while dressing, but none after, and felt as well as usual. He was told to continue the medicine as directed, until thirty drops was reached, and continue that dose until he was seen again. Four days later he was seen, and said he continued until he took twenty-five drops at a dose when the "nervous spells" were so frequent and severe that he could not feed himself and he quit taking it and had no "spells" afterward. When asked what the "spells" were like, he said he did not feel nervous, but acted just like a dog that had been poisoned with strychnin.

He was told to begin again at fifteen drops, and take as directed, and if the so-called spells returned, to report. When

the dose had reached twenty-three drops I had an opportunity to see him in the condition described and his comparison was very apt. Had I not felt very sure that no drug but the iodid had been taken I should have had no hesitation in saying he had taken an overdose of some one of the strychnin group of poisons. Twice the dose was reduced to fifteen drops and then increased one drop a day, but whenever the dose got to twenty-four or twenty-five drops the spasms made it impossible for him to feed himself.

Omitting the medicine for a day left him all right. He could take doses of twenty drops without trouble. The man also proved unusually susceptible to the convulsive action of strychnin, which, when taken in an aggregate amount of one-sixteenth grain daily, would in four or five days produce spasms so similar to those caused by the iodid that he believed he was taking the same medicine in a different form. The only difference I could detect was, that when they had been produced by strychnin, I could reproduce them at will by various means, but when produced by the iodid, they could only be reproduced by electricity, a very weak current always succeeding. At this stage the man disappeared, and I have so far failed to trace him. The particular two ounces of iodid used was not tested as to purity, but the prescription was put up by myself and it was taken from about the middle of a five pound lot made by a reputable American firm, and had answered all requirements of the U. S. P.

F. R. MILLARD, M.D.

San Diego, Cal., November 29, 1896.

ABSTRACTS.

CLEANSING AND CLEANLINESS IN ABDOMINAL SURGERY.*

A few days ago I read the detailed description of an operation for the removal of a bullet lodged in the brain, the operation being performed by one of our best known European surgeons, and a pronounced follower of the school of Lister. He first removed the dressing and exposed the scalp, which looked like a large billiard ball excepting for an ominous bluish spot where the bullet had bored its unkindly way. Then he took a scalpel which was dripping with antiseptic, a precaution that had not been taken with the foregoing bullet, and deftly incising the scalp, while almost all the time an assistant allowed a fine stream of warm water—sterilized by being boiled and allowed to cool to a safe temperature—to play from an irrigator upon the field of the operation. Then instrument after instrument was used (all evidently the subject of pus, for they all "dripped with antiseptic") through the track of the wound, but the locus of the infected bullet was left the prey of the germs which had been carried there and had been working at least forty-eight hours before the operation.

The operator had not read Mr. Ludham Green's interesting papers on how difficult, if not impossible, to sterilize the hands of the operator, for he had consistently made no attempt in that direction, and yet he closed the wound with a pellet of cottonwool fully impregnated with corrosive sublimate. He then addressed his surroundings on the marvellous results obtained in modern times by antiseptic surgery.

In the museum of the Royal College of Surgeons is a large crowbar, technically known as a jumper, which went, under the influence of a charge of gunpowder, from below the chin of the user and straight through his head and out at the vertex, carrying into the wound a lot of germs and leaving them there. No sterilized water was used about the superficial wound and no corrosive sublimate was then employed, about the

*Lawson Tait, M.D., F.R.C.S., Eng., F.R.C.S. Ed., etc., in *Medical Press and Circular.*

end of the last century, yet the patient got well and remained so for years. The museums of this country and of others literally swarm with specimens which prove the receipt of various cerebral injuries with complete and permanent recovery, so that the belief now seemingly established in the minds of surgeons for the present moment that a stream of sterilized water and a few grains of corrosive sublimate in the superficial dressings gives any greater security for recovery is no foundation in fact, and is a mere temporary mental aberration.

Some few months ago I read a paper by the same surgeon. I have already quoted on the subject of the influence of germs and in the short space of a column and a half of an ordinary medical journal is used thirteen instances and phrases as "it is now fully established," "it is beyond dispute," "it must be universally acknowledged," "Smith has proved," "Jones' remarkable observations were established," and "so a complete result of Robinson's original researches we must believe," though in not a single instance would I, for one, admit anything of any one of the single assertions. It happens that those now interested in Smith, Jones and Robinson as three frequent contributors of papers on the application of the ever-advancing, ever-developing, ever-changing conclusions of the bacteriologists to the practical work of the surgeon, and such men, always anxious for second-hand novelties, forget in one week what they said the week before. In their writings, it is an easy matter to picture or detect the extraordinary phases of the evolution of the practice and principle of Listerism, though it is only fair to say that I use Lister's name here with this qualification, that he, whilst the originator and still the chief advocate of the doctrines so various and so varying, is not responsible for more than about half the nonsense which has grown round the original antiseptic religion. There have been a large number of surgical "Pauls" who have freely disseminated perplexing epistles to the various surgical churches of the world, and thereby much and very acrimonious differences have arisen.

The antiseptic generation has now sped its cycle, from 1866 to 1896, and we have come back to the figure of the clock at which we started. The time exactly embraces my own surgical life. In Glasgow and in Edinburgh I saw patients die of the same terrible infliction, no matter what had happened to them. I saw removals of breasts die with a fatality which seemed to rival that of amputation at the middle of the thigh, and yet, in my own practice during the cycle, out of many hundreds of cases of removal of breasts, how many I could not venture to guess, I think it pretty certain that the mortality has been a long way under five per cent., has probably not been one per cent., in fact, I can call to mind only two fatal cases.

The crowded wards, the deficient ventilation, the one salivary dish, the one sponge in each ward, and the wants of ordinary lavatory cleanliness were the causes of the terrible results. The carbolic oil, the putty and the lac plaster may have had some countervailing influence under the circumstances of those horrible old pest houses, but in practice outside such influences they were useless. These details, together with others, which ended finally in the harmonious logic of the spray, marked the first check of the antiseptic cycle, a time during which it was devoutly believed that every germ was potent for evil and every resting spore was a surgical pest. Every germ, every spore, every scrap of harmless dust must, therefore, be submitted to a process of destruction by some potent chemical agent. This chemical agent was constantly changed. So soon as one was contrived, adopted, belauded, and trusted it was found to be wanted when weighed in the clinical balance. The chemical manufacturers were nearly wild, and many of them were ruined by the continual changes, and the antiseptic market rate for years was something more variable than African gold shares.

When the spray was introduced I was led by circumstances altogether outside my own convictions to range myself once more as a follower of the chemical antiseptic school, though I did not for a moment forget nor neglect my old methods. I performed a hundred consecutive ovariotomies with a full and complete adoption of all the antiseptic precautions of the Listerian school of the period, and I published a paper in the "Trans-

actions" of the Royal Medical and Chirurgical Society contrasting the details of that series with those of a consecutive hundred in immediate opposition to them, and the contrast was not in favor of the antiseptic practice.

By this time I had become thoroughly persuaded as to the future progress of the antiseptic doctrines and practice, and I did so in what I called an experiment, though it was rather a satire, that is, I went through all the ceremonious observances with gradually diluted solutions until I used nothing but boiled water, and this was dispensed with. Finally, I used only ordinary tap water, and then I gave up the spray.

This is precisely what has happened all round. The poisonous solutions were mentioned, the spray abandoned, vigorous hunting for germs was advised and the antiseptic belief so modified, that it was lastly accepted that not every spore and germ was benefited, but only such as might yet be identified.

But, on the other side, we found the cubic space allowed to each patient rapidly increased, new hospitals built, and above all, the segregation of surgical patients enormously advanced by the erection of cottage hospitals all over the country. For my own part, between 1878 and 1880, I received an accommodation of about forty beds, for the most part the occupants of which had each a separate room, in fact, practically they may be said to have all had separate rooms. The effect was at once apparent, for my mortality went down from about 30 per cent. to less than five, and I had long runs of fifty, sixty, eighty, and even as far as 146 consecutive operations without a death. Even hysterectomy, the most obstinate of all abdominal operations to yield satisfactory results, has within the last ten years given me runs of thirty, forty, and even forty-five consecutive successes. What are the explanations? The answer is that, though I cannot promise anything from which I can "absolutely prove" or "make it apparent beyond doubt," or anything of the cocksure order, yet I can give basic conclusions which will be with difficulty upset, and those who neglect them will have to bear serious responsibility in the criticism of the future.

The first conclusion at which I arrived concerning abdominal operation was, and it remains the strongest now, that the more the patients submitted to them are separated, the better. For this purpose, and for the greater part of my practice, I adopted and have used a room for each patient. Sometimes with a press of work I was tempted to "pack" as we used to call it, that is, put two patients in one room after the first six or seven days, but I had so frequently to regret this that I ultimately abandoned it.

I am quite sure that there is much truth in one conclusion I have often advanced, that time has much to do with what will happen in the septic infection of an abdominal section. The fourth night is the critical night with all save hysterectomies, and with them that is not to be so definitely fixed. If an ovariotomy is all right on the fifth morning the chances of her going wrong are small indeed. But if you "pack" them they will have hematoceles, stitch abscesses, pulmonary complications, mumps, and all sorts of secondary troubles, in proportion far greater than if you keep them absolutely one in each room. These complications do not affect the mortality much, but they prolong the convalescence in a fashion of the most annoying kind. This has been still more impressed on me during the last three years, in which I have far more widely adopted the plan of operating in the houses of the patients and leaving the subsequent treatment of them to their private medical attendants. This is now possible, seeing that I sedulously avoid anything in the shape of gratuitous work, whereas, for more than twenty years I did not get payment of any kind from more than one-fourth of my clientèle, and not more than costs out of pocket from one-tenth. I am now, therefore, working solely in a class where it is possible to have all that is requisite in the way of accommodation in the houses of the patients; and after the operation is over, I am seldom required to see the patients again, recoveries are so little interrupted. All this experience points out to me the extreme importance of segregation and the uniform results of the work at the Sparkhill Hospital, not in the hands of one man, but in the hands of all to whom it has been entrusted, proves this

as far as proof in surgery can go, for segregation is carried out there almost as completely as it can be carried out in the best houses, whilst the perfection of the sanitary arrangements provided by the Committee of Management are almost absurd in their completeness of detail.

All this and the necessity for it enforced on my mind connected with the complete banishment of every fear of germs, either wholesale or collective, though there still remained with us the wholesome dread of certain specific poisons the nature of which I do not know, and I think I may safely say that their nature is equally unknown to everybody else and first of all of them, as deadly beyond all things known, that is, the poison begot in the peritoneum and uterus of the puerperal woman, and in some cases dying after abdominal sections.

This brings me to speak of the third phase of the slowly-developing Listerism or antiseptic doctrine, when it has begun to call itself aseptic and to adopt some of the known doctrines and practices which I have been practising since 1881. All that I have been saying up till now leads me to that condition which, after segregation, constitutes my surest general condition essential for success in abdominal surgery and going a long way to explain our modern success—I mean cleanliness.

Cleanliness in surgery may be divided into general and specific. General cleanliness, such as close attention to cleaning wards and all in them, the cleanliness of all linen, bedclothing, and the personal cleanliness of the surgeon and his members of his staff, are matters I need not waste time over. The details of specific cleanliness are matters much more in need of discussion.

For a long time in the earlier part of my practice, I allowed all properly introduced and qualified practitioners to visit my practice, and they came in great numbers, chiefly from America. But I soon had to stop this kind of hospitality and to limit admission to such as came as serious-minded students, prepared to see and understand what they saw. The reason for this was sought, the "globe-trotter" came and saw one or two operations and departed without understanding anything he had seen and, if he published, perversely misrepresenting the facts. To this mistake is due two misstatements which even now appear at intervals in the medical journals of the Continent and America. The first is that I really am a devoted disciple of the chemical germ-destroyer school, but that I have some substance, one which I will not disclose. The second is that my secret is "water sterilized by boiling," and this ridiculous misstatement recurred only a few months ago at one of the great congresses of America, and from the mouth of one of my old pupils, Dr. Ricketts, of Cincinnati, who spent six months with me.

It is now fourteen years since I have used sterilized water for any purpose save to raise common tap water to the temperature required, so that the mixture would be probably five parts common tap or well water and one part water which had been boiled and thereby sterilized. The mixture would have a temperature of about 102 deg., for my hands will not stand much more with comfort. I have yet to learn that such a mixture deserves the term of "sterilized."

My attention to specific cleanliness is as close as can be given. It may be divided as between the instruments and hands of the operator and the abdomen of the patient.

I hail with great satisfaction all the wonderful inventions and devices of the modern operating theatre, for securing cleanliness, for that cleanliness can be secured only by the work of women, and women in themselves have not the slightest idea of cleanliness save on the surface, and unless they belong to the really well-educated classes. This is a fact which no one knows so well as he who has gone through the filthy drudgery of a gynecologic out-patient department, when fifteen out of sixteen of the patients have lice or fleas upon them, and often both. This is the material from which the great bulk of the so-called trained nurses are obtained, and with this training, unless most especially well looked after, they alter their habits only in the sense of the smart cap and the attractive uniform. They remain as dirty as ever, and it is therefore necessary to give them shelves and boxes of plate glass.

All instruments with sliding tubes, screw or bended joints should be abandoned, every joint should be capable of being unshipped, and after every operation every instrument used should be scrubbed with raw turpentine and a brush, and then well washed with soap and water. If this be done, immersion in cold tap water for the *next* operation is all that is wanted.

Sponges—ah! they want a paper to themselves! They are, and ought to be, the terror of the operating surgeon, and I cannot stay now to say what I have to say about them, save that new sponges are bad, old sponges are dangerous, and that none of them should ever be boiled. The Americans will have it that I boil my sponges, but I never did such a thing but once and that ruined the lot.

Now I come to the real subject-matter of my paper, the cleanliness or cleansing of the patients.

I see that a number of superstitious observances on this subject are still recommended, such as the application of an antiseptic pad to the abdominal wall for twenty-four hours before the operation, etc., etc.

I have never employed any such plans, being quite content with a soap and water washing of the skin to remove the dead fat and epithelium with which women are always coated and generally thickly. If there were any real poison in the skin no antiseptic pad would remove it in twenty-four hours. The real poisons known to me as absolute realities, are those of which I have spoken as occurring in puerperal peritonitis, cannot be removed by any known germicide from the hands of the surgeon infected by them. Mr. Ludham Green's experiments seem to show that it is impossible to cleanse the hands from the ordinary spores of decomposition, and yet we know that nowhere is epithelium reproduced and shed at the rate it is on the hand. My knowledge of the terribly infective power of the puerperal poison, from my own experience and that of others, has been so emphatic, and the lessons so disastrous, that I am persuaded that the poison, whatever it be, permeates at least the whole epithelial layer and cannot be got rid of, save efflux of tissue and skin, and that it is not safe for anyone so infected to operate till at least a fortnight or three weeks have elapsed. Have there not been lessons enough in the same direction by the spread of puerperal fever by the hands of the *accoucheur?*

I am not alarmed at the conclusions to which Mr. Ludham Green's observations point, for I do not fear the ordinary germ poisons at all. But still I take the precaution of keeping my nails short and clean, and of washing my hands in raw turpentine the last thing before performing any operation and then washing off the turpentine by ordinary soap and water. My reasons for this may be seen in the simple experiment of washing the hands three or four times in the ordinary way, and then in perfectly fresh water, repeating the process with the addition of turpentine. After the water has stood for a few minutes there will be on its surface evidence of a dirt of a very convincing kind. That dirt must be either in the clear turpentine or it must be a layer of dirt removed from the hands by the turpentine which resisted the previous efforts with soap only. The latter conclusion is that accepted by me, and it accounts for my using turpentine to the patient's skin and my own, as well as to my assistant's, in the rare cases when I need one.

Thus great cleansing, and I think by far the most important of the lot, is the cleansing of the abdominal cavity during and after operation.

Mrs. Flinders—John, you must put an advertisement in the paper for another girl to-morrow morning. Mary is in paradise, now.

Mr. Flinders—What! Did she start the fire with coal-oil?

Mrs. Flinders—No; she's got a chance to work in a crockery shop.—*Indianapolis Journal.*

"Jarley, the editor of the *Hustler*, is an absent-minded fellow." "How's that?" "He was dining with a friend the other day, and when asked if he would have some pudding he replied that owing to a tremendous pressure for space he would be compelled to refuse." *Washington Times.*

SOCIETY REPORTS.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

Stated meeting, December 9, 1896.

The President, DR. J. C. WILSON, in the chair.

DR. EDWARD MARTIN presented a communication entitled

"AMBULANT TREATMENT OF FRACTURES OF THE LEG,"

and demonstrated the method of applying the dressing, also exhibiting patients. The method of treatment consists in so applying the plaster dressing that a patient, instead of being confined to bed for five or six weeks, can get about and attend to any light occupation. The dressing had been applied to twelve patients in the last six or eight months, and it was hoped that all would be present, but only five were able to come. Dr. Martin has employed the dressing in upwards of thirty cases and the results have been good. There were two cases in which the results were not thoroughly satisfactory, but in these Dr. Martin did not apply the dressing personally. One was an instance of Pott's fracture. The deformity was not greater than is often noticed after treatment by the fracture-box, but there should have been none. The other was one of compound fracture, for which the patient was operated on. The wound healed kindly at first, but later it broke down and the fracture was found to be ununited. The bone was then cut down upon, and a portion of the tibial shaft, about two inches, resected. A few days after the operation, as the operation-wound was running a sterile course, Dr. Martin directed an ambulant dressing to be applied. The patient was up and walking five days after the operation, and he apparently did well, the bone uniting, although there was some deformity. Dr. Martin has never used the dressing for fractures of the bone above the knee, and with the exception of the last case all have been simple fractures.

The first patient exhibited was a boy, who was run over by a bicycle, and both bones of the leg were broken at the middle third. There was no difficulty in replacing the bones in good position, and an ambulant dressing was put on a few days after the injury. After the plaster was applied there was no pain, and the patient has been walking ever since—that is seven weeks ago. There seems to be less stiffening and after-trouble than is common when the fracture-box is employed. The boy has been very comfortable and has been saved a long confinement in bed. The first dressing applied broke, because of poor plaster, and a second one was put on about ten days later.

In the second patient, a boy, the first dressing was poor and a second had to be applied. The latter was worn for five weeks. The union is perfect and there is no disability.

Dr. Martin explained that he was not advocating this method of dressing particularly; nor was he trying to prove that it is better than any other dressing. He merely wanted to decide what its real merits are, and he was simply taking testimony.

The third case was one of fracture of both bones of the leg at the middle third, during a fight. The patient began walking the day after the application of the dressing. He has been coming to the Dispensary to be dressed, and has been saved several weeks in bed.

The fourth case had a Pott's fracture; the patient began walking the next day, but had some pain.

The last case was in an old woman, who fell down stairs some three or four months before, breaking the upper third of the tibia and fibula. There was very little deformity, and the dressing was applied promptly. She had less use of the dressing than any patient that Dr. Martin has thus treated. She could not walk without a crutch or chair. She did not go home for three weeks. She did not complain of pain, but did not seem able to manage the large, heavy dressing. The day following the application of the dressing, the second after her accident, Dr. Martin took her by the hand and led her up and down the ward, but the walking was not rapid or easy. Perhaps the best thing the dressing did for this patient was to shorten her stay in bed and allow her to go home and walk about one or two weeks sooner than she otherwise would have done. She presented absolutely no deformity; nor did any of the cases.

The cases presented were not selected by any means, and represent very clearly what can be expected from the dressing. The first proposition to treat fractures by the ambulant method was made by Bardeleben, in 1894, and following him, a number of German surgeons tried it. Bardeleben reported upwards of 111 cases. Some of his colleagues reported a number of others, so that the total number was 200. Since that time Dr. Martin has had upwards of thirty cases of his own.

The Germans apply the ambulant dressing not only for fractures of the leg, but also for simple and compound fractures of the thigh. Some students of Dr. Martin's in Germany at the time of Bardeleben's first statement reported that the results in fracture of the thigh were unsatisfactory, marked deformity often resulting.

The princip'e on which this dressing is applied is that of fixation and shifting

of support. Given a fracture of one or both bones of the leg, as soon as the weight is thrown on the foot there is, of course, a tendency to overlapping of the bones. If the weight of the body could be carried from above the fracture to the ground by external supports, as by means of a plaster case passing from the knee-joint to below the sole of the foot, the tendency to overlapping would be at once corrected. If, at the same time, the weight of the body could be not only kept from causing overriding of the fragments, but could also fix the fragments in good position, there seems to be no good reason why the patient should not use his leg. This is accomplished by the so-called ambulant splint; the plaster is so applied that the weight of the body is carried from the tibial tuberosities just below the knee, to the reinforced portion of the dressing lying below the sole of the foot, pressure upon the malleoli being avoided by abundant cotton padding. This dressing fixes both the joints contiguous to the fracture. It is, perhaps, open to a serious objection, as it entirely prevents the use of massage. It is inapplicable to certain cases, *e.g.*, when there is great swelling, when the shape of the leg is such that the weight cannot be carried from the tibial tuberosities to the rigid dressing that envelops the leg (this is particularly the case in very fat women with small bones). In most instances of simple fracture of the leg, when reduction is possible, either with or without ether, and when the swelling is not marked, the dressing is the most satisfactory that can be used.

The dressing should be applied as soon after the fracture as possible. It requires abundant padding with some elastic material. It must be so arranged that the plaster does not press into the malleoli, and the plaster must be neatly fitted below the knee; it should be worn from four to six weeks. On the day following the application of the dressing, the patient is gotten up, with the help of an assistant or a friend, who takes him under the arm and assists him in walking up and down the ward. The Germans use an apparatus very much like the walking device provided for children, the patient standing inside a sort of cage which is on wheels. In a day or so walking is possible with two crutches; in another day, with one crutch, and in a week the patient can usually get along with a cane or without any support. It is scarcely rational to apply this dressing to a hod-carrier and expect him to be able to continue his employment at once; but a man can attend to ordinary business pursuits.

Another advantage of the dressing is that less atrophy follows than after the use of the ordinary fracture-box or plaster dressing. Dr. Martin held that the fracture-box is a poor dressing. Even if the patient is lying in bed the plaster is best. The atrophy is a manifestation of a reflex neurosis as has been proved by experimental research. The great advantage inherent in the plaster dressing is that it enables the patient to be up and about and that anyone who realizes how irksome it is to stay in bed when it is not absolutely necessary may perhaps save these patients four or six weeks of lying on their backs.

As to the direct method of applying the dressing: In the first place the plaster must be fresh and that is often a very great trouble in plaster bandage. They are better made from a coarser mesh of gauze than that used for the ordinary surgical bandage. A lot of cotton batting should be provided and the fracture must be reduced. A mistake is often made in not etherizing more frequently than is done in the treatment of fractures. The first essential is thorough reduction. When there is swelling and the patient is sensitive there is a great temptation to trust that reduction is accomplished without making this perfectly apparent. In perhaps the majority of cases in applying this plaster bandage Dr. Martin gives ether. He always does this when it is not absolutely certain that the bones are not in proper position. The leg is then enveloped in a flannel roller, as is customary in the application of all plaster dressings; the cotton is wrapped about and then the plaster is applied.

For the purpose of flanging out the lower part of the plaster and giving a little more strength, Dr. Martin has had made some slightly flexible steel rods, such as are used in orthopedic work for bracing shoes. They are flexible so that they can be molded to the desired shape. In putting on the dressing it is essential that the toes should point a little upward. If they drag they are liable to strike things. The heel should be a little lower than the toes. A child of eight or ten years of age requires a flannel bandage two inches wide. The foot is covered in, no reverses being employed. In putting on the cotton batting it is well to tear it off the required width and it then rolls out very much like a bandage. The malleoli are thickly padded, so that the plaster may be prevented from falling in and pressing at these points. At the knee-joint where the plaster should bear pressure the batting is thinner. If the surgeon intends to take the plaster off himself it is well to partly cut it and for that purpose a lead strip is needed. The sole of the foot is padded with a mass of cotton at least three inches thick. The plaster is now ready for application. The first two turns take in the padding on the sole and keep it in the middle line. In applying this bandage a little tighter pressure is employed than with an ordinary plaster bandage as, because of the thick padding, the parts will stand it.

When the patient is placed under the influence of ether, he must be kept under until the dressing is hard. The bandage runs up to a little above the knee. Having closed in the cotton the external metallic splints are applied. These are molded to the part and covered in with more plaster. The patient is kept perfectly quiet and the toe is held up until the plaster hardens. In another twenty-four

hours the patient is allowed to put the foot to the ground. While the plaster is still moist a knife is run along the line of lead strip, leaving several bridges, which should be marked by pencil before cutting. When the time comes to remove the plaster the bridge is simply cut through by a penknife and the dressing will spring off. Even plaster dressings that have not had this preliminary cutting can be best removed with the penknife. The leg sas they come out of this plaster show some wasting; the ankle is stiff, but no more stiff than it is in the fracture-box.

DISCUSSION.

DR. ERNEST LAPLACE stated that the method demonstrated impressed him as being a very good one, especially for such cases as lying in bed would be injurious to. It is to be preferred to other methods, because it allows the patient to go about and to preserve his ordinary health, instead of being weakened by being kept in bed. It should be applied in every case in which the patient would otherwise suffer from being kept in bed. All patients when kept in bed will be lowered in vitality especially children. When children are kept going about, the nutritive processes are maintained and the formation of callus is more rapid. The treatment has the further advantage that if the leg is well extended while the splint is being applied there is no danger at any time afterward that contraction from undue irritation of the muscles in the neighborhood of the fracture will result, and there is less likelihood of a deformity or of imperfect union at the point of the fracture. Whatever may be said of the fracture-box it is virtually an open dressing, and the surgeon and his assistants are constantly tempted to look and see if the fracture is doing well, with the result that the muscles in the neighborhood will be irritated and tend to contract. If it is possible at first to put a dressing on that will almost positively hold the bones in apposition and that will be permanent, that is the dressing to be preferred above all others; and should that dressing furthermore allow the patient to walk about, it is the ideal dressing.

DR. OSCAR H. ALLIS said that the oldest treatment on record and one that has never been reversed is, that in regard to fractures, there are two fundamental principles to be followed: first, to replace the fragments; second, to immobilize them. Dr. Martin has shown that these ends can be effected by the ambulant dressing as well as by the fracture-box. As to the important question of getting about, Dr. Allis expressed a fear that if a person had an ununited fracture or bad deformity following any new method of treatment, such as the one described, and were to go into the courts, the outlook for the surgeon would be rather dubious. The dressing of fractures with the ordinary plaster-of-Paris bandage has been long and favorably employed. The ambulant dressing need not interfere with this, as it may be made to take its bearing from a point above the fixation dressing. Dr. Allis further pointed out that in order for the patient to walk as conveniently as possible the foot is placed at right angles to the axis of the tibia; under these circumstances the stretching of the calf-muscles and the relaxation of others may result in overlapping of the fragments.

DR. WILLIAM J. TAYLOR said that if Dr. Martin had appreciated the amount of destructive change in the bones in the case specially referred to he would not have attempted treatment with the ambulant dressing. The case was not a suitable one; and the disease was very extensive, with overlapping of the fragments and a great deal of destruction of the bone. The patient was not seen until some time after the fracture. The site of injury was cut down upon and the bones were found very much displaced. The fragments of the tibia could not be brought together at all because the bone was broken at the lower end; consequently there was no possibility of good union. There was also a great deal of destruction of the tissue there.

DR. JAMES M. BARTON stated that he had used this method of treatment in some four or five cases at the Jefferson Hospital. In none was the femur fractured, but in all the tibia and fibula were involved. The results have been fully satisfactory in every way.

In the first two cases some anxiety was felt, and the patients were kept in the wards during the entire treatment. The dressing was removed in the middle of the treatment, but the apposition being found satisfactory the dressing was reapplied. Four strips of rather heavy tin, roughened to prevent slipping, were used.

If the patient in stepping puts his weight on the toe, he will soon break the cast at the ankle. To avoid this, it is usual to bend the foot at the ankle, making the patient walk on the prominent heel. Some patients have found this position very irksome, and the dressing, when it is about finished, may be completed by putting a mass of cotton, about the size of the fist, under the heel and covering it with the plaster bandages. By this means the patient is slightly elevated; his toe cannot readily come in contact with the floor, and the foot is comfortably maintained at a right angle.

DR. G. G. DAVIS said that, in most cases, the results of fractures are in direct proportion to the care exercised in treating them. The question suggests itself as to whether, in a difficult case, the means of treatment demonstrated would be as efficacious as other means. Dr. Davis believed not, although admitting that it was not brought forward as a means of treating all classes of cases. While the ambulatory treatment of fractures is a decided advance in surgery, and while progress in the treatment of fractures will be made in that direction, the question as to the best means of doing this has not yet been settled.

The next question is as to the time of application of the dressing. Shall a patient be immediately put upon an ambulatory treatment, or later? Personally, Dr. Davis believed that the best results will be obtained in nearly all cases by not putting the patient upon an ambulatory treatment until about eight or ten days after the bones have been broken, when the parts will have had time to glue themselves into suitable position.

Dr. Davis does not believe in the treatment of fractures solely by plaster-of-Paris, notwithstanding the assertion that plaster-of-Paris only gives unsatisfactory results in unskilful hands. It is an untrustworthy dressing to use from the first. Displacements can occur beneath the plaster, and these may remain unrecognized, to be discovered only upon the removal of the plaster. Therefore, it is preferable not to use any dressing that will prevent the surgeon from assuring himself that the fracture is in a good position. In some cases, in which the injury is comparatively slight, the tendency to deformity is almost *nil*, there being no displacement and practically no tendency for it to occur. Such cases will cure themselves under almost any plan of treatment. Ordinarily, the simple pain that is produced in walking or in attempting the use of a fractured limb will induce the patient to refrain and thus avoid appreciable displacement.

In the case of a patient under the care of Dr. Davis, with an undoubted fracture of the fibula and swelling of the ankle-joint, the limb was in a few days placed in plaster-of-Paris. Two weeks later pain was experienced, and on removal of the dressing it was evident that the tibia had been fractured obliquely through its outer half into the ankle-joint, and the traction of the tendo Achillis had drawn the heel upward and tilted this fragment forward. The position of the foot was apparently not changed in the plaster; it was only the pain that caused the dressing to be examined. Had this been a case of ordinary oblique fracture of the leg, the displacement would probably not have caused much pain, and the patient would have been allowed to have his leg remain in the splint, and the consequence would have been a deformity.

The field for ambulatory dressings for fractures is limited. While eventually the use of ambulatory dressings will become more general, and is in the line of progress, it is hardly as important as it is thought by some. It is comparatively easy to get brilliant results in children by the ambulatory method; so it is likewise by other methods. That a child can crawl around the floor with a fracture-box attached to its broken leg, is evidence of how little disturbance a fracture may cause. It has not been Dr. Davis' experience that a child's health runs down when it is kept in bed. On the contrary, children can live in bed almost indefinitely without their health deteriorating, and in tuberculous diseases at all events the health may improve. Therefore, the ambulant treatment is not so necessary with them. In many cases in adults there is no desirability of allowing a patient with a fracture of the leg, for instance, to use the affected extremity (the experience of the Germans has sufficiently demonstrated that the ambulant treatment of fractures of the thigh is not desirable, and the method is confined to fractures of the leg). The majority of people with fractures of the leg are not desirous, particularly in hospital cases, of at once resuming their active duties. Therefore, a certain period of recumbency or rest is not objectionable to them.

However, there are some cases in which it is desirable to get a person up and about as soon as one possibly can, and in these a dressing such as that shown is an excellent thing. Other means of achieving the same object would be the use of a high shoe on the opposite foot and crutches, the support of the leg being made by a dressing of sodium silicate, or other immovable dressing.

The use of various forms of apparatus was likewise advocated some time ago, and more recently again by Bradford, of Boston. Another very good way, and one of the most reliable ways, of achieving this object is by the use of the Thomas knee-splint, in which the support is taken upon the tuberosities of the ischium by parallel bars along the side of the leg. Surgeons are not yet qualified to state positively which is the best method of treating fractures; neither does the patient suffer much in the large majority of cases from his confinement on account of his fracture, but if the surgeon shows too great an anxiety to get rid, so to speak, of personal supervision of the case, in just that proportion will he be likely to find his results deteriorate.

DR. J. P. MANN said that he has used the ambulatory method of treating fractures several times, and is not convinced that it is the best method of treatment for these cases. As to the use of plaster-of-Paris in the treatment of fractures, there is not a bone in the body, except the skull, that he has not applied that to. In *The Medical News*, July 15, 1893, p. 70, Dr. Hearn and Dr. Mann reported seven cases of fracture—four of the femur, two of the humerus, and one of the radius and ulna—treated with plaster of Paris and splints. There was in two of these cases a quarter of an inch of shortening. One of the cases was in an old lady, seventy-eight years of age, who fractured her femur just above the knee, making a compound oblique fracture. The wound was immediately asepticised, the bone replaced, and plaster-of-Paris put on from the toes to the arm-pit. The woman lay in bed for eight weeks with very little discomfort. The result is to-day yet perfect. In all of the cases reported the results were good.

There would be some danger in a case of oblique fracture of the tibia in the use of the ambulatory dressing. The amount of cotton used about the foot and ankle seems to permit more motion than is desirable, and overriding of the bone would very probably take place. In

one of the children presented by Dr. Martin there seemed to be considerable backward curving of the tibia and fibula. In another there was considerable bowing outward of the leg. This, of course, might have existed before, although not evident in either of the unfractured legs. Of course, such results attend also other forms of treatment. It is preferable to use for all such fractures the usual plaster-of-Paris splint. In a fracture of this kind this splint is especially applicable because, during its application, extension and proper apposition of the parts can be very readily maintained. In case of a fracture along the shaft of the tibia, for instance compound, as that is the kind most frequently desired to be seen, a section of the plaster-of-Paris dressing is first placed above the seat of fracture and then another below the seat of fracture. Across the seat of fracture and along the sides of the leg are placed two strips of iron, which are incorporated between the layers of plaster. Thus the space can be bridged over, and then before fastening these lower two ends of the irons, which have been fastened above, an assistant makes extension upon the foot to put the bones in proper place. When the plaster hardens, it is impossible for the leg not to remain in extension. The wound is then readily accessible for dressing. Mostly, it is possible at once to cover the whole limb as far as necessary with plaster-of-Paris. If there is considerable swelling, absorbent cotton is used freely, and then, after the swelling has subsided, the first plaster dressing is removed and another put on that will fit more accurately.

No plaster-of-Paris dressing can be considered good unless it hardens within five minutes. Formerly, one of the strongest objections to plaster-of-Paris was that it took so long to harden. It was discovered some eight or ten years ago that if the materials used in plaster splints, as well as the materials used underneath the plaster, would not readily absorb water the plaster would harden slowly. Since absorbent lint and buttercloth (not cheesecloth) have been used it has been found that with a little salt put in the water, which should be slightly warm, the plaster can be made to harden within five minutes. In fractures in any part of the body away from the skull no dressing will do the work that plaster-of-Paris does. It is now being used extensively in hospitals, and as surgeons are becoming more accustomed to it they find no difficulty in its application. It can be removed very readily with an instrument devised by Dr. Reed, of the University of Pennsylvania. It was formerly removed with a penknife or saw, but Dr. Reed's instrument has greatly simplified its removal.

Dr. Edward Martin said that the possibility of producing deformity by bringing the toe up had occurred to him, but it did not happen to take place in the cases in which the dressing was used. The best dressing is that which does the work best.

As for the confinement in bed, it might be a good thing if all children could be kept in bed from the time they are five until they are twenty-one, but it seems scarcely a good thing to put children to bed if it is not necessary for their own comfort. It is rather better to keep them upon ambulant treatment if it can be done. If the amount of bone removed in the case operated upon by Dr. Taylor had been known, the ambulant treatment would probably not have been used. Under any circumstances it is doubtful whether the result was any better or any worse than would have followed any other method of treatment. The man returned to work, but, as often happens about the seat of old compound fractures, there developed apparently some necrosis one or two months afterward.

The material into which plaster is rubbed is a subject of indifference, provided that its meshes are of sufficient size—its absorbing power is dependent upon the removal of fat, a process to which most fabrics used in the preparation of plaster bandages are subjected.

Dr. Thomas J. Mays read a paper upon

The Local Application of Cold in Acute Pneumonia, Being the Third Collective Report; With a Discussion of Fever in this Disease.

(See page 767.)

Dr. James Tyson said that his own experience in the treatment of pneumonia with cold has been rather limited, but so far as it has gone it has been satisfactory, so satisfactory indeed, that he has concluded for the present, at least, to treat, in this manner, all cases of pneumonia associated with high temperature that may come under his care. The attempt to arrive at a rational explanation of the operations of cold in these cases seems to be attended with difficulty so great as to make it scarcely worth while to spend much time upon it in a brief discussion. It is well known that the fever is a symptom secondary to some potent cause, such as a vegetable organism or a toxin generated by it, while the fever is an index in some way of the seriousness of the operation of this cause. It is also likely that the treatment by cold is useful in pneumonia exactly as it is in typhoid fever, while the high temperature itself, being less prolonged, is *per se* less dangerous. The most desirable stage to reach in the practice of this matter is to get over the fear of any possible harm to the patient from the cold, for such fear is entirely unfounded. It is not at all a new doctrine that it is impossible to do harm by cold to a patient who is in a state of fever, as long as the fever lasts. It matters not much how this cold is applied, whether by cold baths or by opening windows and allowing the cold winter air to blow upon the patient. There is a certain advantage in treating pneumonia by the direct application of cold to the chest. One other part of great importance in this treatment is to discontinue the cold as soon as the temperature approaches the normal. It is also well

not to use the cold in those cases, especially met with in old persons and in alcoholics, in which there is no elevation of temperature.

Dr. J. M. Anders said that in the discussion that followed the reading of Dr. Mays' former paper on the same subject he stated that he had used ice locally in a couple of instances of pneumonia with encouraging results. To that experience he has added half a dozen cases, in all of which the effect of the cold was also very good. In no case did the cold applied locally abort the disease, but the general course of the cases seemed to be somewhat milder than under other methods of treatment. The temperature ran a lower course and in three of the cases it seemed that the area of consolidation remained more distinctly circumscribed than in cases otherwise treated, although this may have been a coincidence. Dr. Anders has not, however, used cold locally as a routine measure; nor did he agree that cold is applicable to all cases of acute pneumonia. The cases so treated were selected. The patients were not infants, nor very old persons; they were not individuals who had been previously much debilitated or who possessed markedly sensitive organizations. If the advice laid down by Dr. Mays were followed rigorously accidents would be common. Occasionally instances will be encountered in which the cold will do more harm than good. Dr. Anders referred to a case of lobar pneumonia seen in the practice of a colleague in which neither the poultice of ice nor cold compresses could be tolerated. The patient was a lady about sixty-two years of age and seemed to have a very highly sensitive nervous system.

In all cases of pneumonia, particularly in the first stage, or that of engorgement, there is apparently a clear indication for the use of cold locally, as it is known that this agency causes contraction of the dilated capillaries and thus relieves or scatters congestion even. Now, whilst admitting these facts, and whilst believing that in the vast majority of cases cold should be used, still it should not be employed as a routine measure.

Dr. H. A. Hare agreed that cold should be employed in certain selected cases of pneumonia; there is not a single measure that can be employed as a routine means of treatment in every case, and the difference is very marked between the application of cold in a disease like typhoid fever, and the application of cold in a comparatively short-lived disease, such as croupous pneumonia. Physicians, when called upon to treat a case of croupous pneumonia, are inclined to be therapeutists instead of being watchmen, and to use remedial measures all the time rather than when indications for them arise. After the stage of congestion has passed and the second stage has been entered upon, indications must be met and combated as they arise. The damage is then done, and we must aid in clearing up the wreck. A temperature of 103° or 104°, which only lasts for a day or two, does not exert any particular deleterious influence upon the patient. If it goes very much higher than this and comes to be a hyperpyrexia, then the question of heat-stroke must be considered. Below this, it is simply a manifestation of the disease, and the fever does not call for medication by drugs or treatment by external remedies. It is not likely that cold, locally applied to the chest, yields all the good results that have been claimed, over and above that of cold in the reduction of fever. In the early stage of pneumonia an attempt should be made to relieve congestion by appropriate measures, but if the lung is wrecked by exudate, it seems that cold will be of little service. On the whole, when the stage of consolidation is very well developed, the continuous application of cold would not necessarily result in resolution and absorption, and it is necessary to treat with care cases of pneumonia in which, either because of inherent feebleness or delicacy of the system, cold is contraindicated. The continued application of cold to the patient's chest, up to the time of crisis, may do exactly what the physician so anxiously tries to avoid, *i.e.*, produce a sudden fall of temperature from 104° to below normal, with the patient almost or entirely collapsed. One of the most important things that could be done would be to discover the limitation of the usefulness of cold to certain cases of pneumonia possessing certain peculiarities, and to see whether it is not possible, by carefully going over the statistics, to limit the application of cold to some particular stage of the disease. It can be imagined, for instance, that when pericardial complications attend pneumonia, an ice-bag might do good, just as it does good in some cases of ordinary uncomplicated pericarditis.

Dr. S. Solis-Cohen said that if statistics might be relied upon to prove anything as to the treatment of any disease by a special method, the statistics presented would at first seem to prove Dr. Mays' contention, but cases of both croupous and catarrhal pneumonia differ so much in origin and in course, as well as in the complications with which they are attended, in the age and constitution of the patient, in the time and circumstances under which they are manifested, that it would be utterly impossible for any one plan of treatment to answer in every case, any more than one could expect a coat, no matter how good its texture or how beautiful its fashion, to fit every person in a large gathering. On the other hand, the statistics of Fenwick from the records of the London Hospital, which include one thousand cases of acute lobar pneumonia carefully studied, and in which all the concomitants, the ages and comparative vigor of the patients, the general and special symptoms of the cases, the complications, the diseases, if any, upon which the attack of pneumonia has supervened, the time of year and other modifying circumstances, have been considered, show one thing clearly and that is the usefulness of fever and fever-heat in pneumonia as nature's method of bringing about

recovery. It appears from these statistics that thirty-five per cent. of all deaths coincided in time with the sudden deferescence, the only period more fatal than this being that of the height of the fever. Those cases attended with an average temperature of 103° F. presented the minimum of mortality, the states of apyrexia and of hyperpyrexia corresponding with the maximum of danger to life.

In other words it is the extremes that are to be dreaded, whether of high temperature or of low temperature. These views are not novel or on trial. They are the old Hippocratic doctrine stated in modern language, and it seems that this doctrine, namely, that certain elements of the febrile complexus, and especially the febrile heat, being the reaction of the normal organism against the invading hosts of disease, are to be controlled but not antagonized, is thoroughly established. Notwithstanding advances in laboratory methods a great deal is still to be learned from the old clinical observers who studied their cases carefully and who took note of all of the environing circumstances, while with all the instruments of precision there exists the liability to overlook important facts. Careful reading of the teachers of medicine throughout all time will show that this doctrine of the usefulness of fever as nature's reaction against causes and processes of disease has been held by those whom we may properly regard as leaders of medical thought, and none of the recent exact investigations has tended in any degree to overturn that doctrine. It may not be easy to understand it or satisfactorily to explain its mechanism, but it is an absolute clinical fact.

It would be preferable to see a patient with lobar pneumonia run along with a temperature a little higher than one a little lower than the average. One could be more hopeful of a favorable result. It would indicate not so much the severity of the disease as the energy of the salutary reaction. On the other hand hyperpyrexia is unquestionably an indication of danger; it shows not an energetic reaction, but an unbalancing of the regulatory mechanism, perhaps as a result of profound intoxication, perhaps of constitutional weakness—but these are speculations upon unsolved mysteries. Dr. Cohen has seen a number of cases of pneumonia in which cold applications have apparently done good; a few in his own practice, more in the practice of others. He has seen some of Dr. Mays' own patients at the Polyclinic Hospital, which seemed to do remarkably well under the influence of local cold and who seemed to be pretty comfortable with it. If any general rule in these cases could be laid down it would be to use cold applications whenever they added to the comfort of the patient. They would then be likely to prove useful; but whenever they added to his discomfort they would be likely to prove harmful. Hot local applications in pneumonia have been used from time immemorial; and are to be preferred, if they seem to give relief. The same rule applies and the effect is probably very similar.

Dr. Ernest Laplace maintained that the pneumonia produces fever, or that fever in pneumonia is a direct result of the amount of toxin remaining in the blood, because of the development of the pneumococcus in the blood, and that there is fever in a direct ratio to the toxin present. This toxin is a chemic substance and it passes through the blood to the thermogenic centers at the base of the brain. These centers are irritated and the result is fever. That is no mystery, but a scientific fact which can be reproduced, not with the toxemia of pneumonia very well, because that is beyond any crude analysis of the chemist to-day; but pneumonia with fever can be produced by any chemical at will, say with compound tincture of iodin, as a direct result of the irritation of the base of the brain. Most violent diseases, whether surgical or medical, accompanied by high fever, have a tendency to recovery and those which are accompanied by less fever have less tendency to get well. In other words, when there are great disturbances of the body, the resulting variations of temperature will so alter the soil of the system as to render that soil itself deadly to the invading germs and they will die spontaneously and the patient gets well. Tuberculosis of spinal origin or of the hip-joint is at no time attended with the production of sufficient poison to alter the soil and make that soil itself death-dealing to the germ, as in pneumonia. As a result the body is slowly worn away and the patient dies. On the other hand the tetanus-bacillus develops quietly, but its toxin passes through and irritates the brain; it acts like strychnin, which, unlike compound tincture of iodin, will not induce fever, but simply causes violent spasm and as a result the soil is at no time death-producing to the germ. In other words, the progress of pathology, medical or surgical, is directly in the line of the reaction of the tissues of the brain, or of other organs, to the products of micro-organisms and the same resistance of the tissues of the body to the various toxins exists as between the tissues of the body and various drugs used.

Dr. J. C. Wilson expressed regret that in the collection of cases reported and their analysis the distinction between croupous pneumonia and broncho-pneumonia—two entirely distinct diseases—has not been made out. He emphasized the statement that the height of the temperature is no index of the severity of the process. Well-developed croupous pneumonias, beginning abruptly with high temperature, run, on the whole, a course usually more favorable than those in which the temperature does not rise above 102.5° F.

Dr. Wilson has been in the habit for some years of using applications of ice to the chest in the manner described by Dr. Mays, in the treatment of croupous pneumonia both in private and hospital practice. There is no reason to suppose that the application of cold to the

chest has any direct action upon the blood-vessels of the interior of the lung. It is certain, however, that it relieves pain to some extent, as well as lowers the respiration and to a trifling extent reduces the temperature. There is no ground for believing that it at all limits the extent of the exudate nor has a most careful study of the subject shown that it hastens the crisis.

DR. J. P. CROZER GRIFFITH said that the treatment of pneumonia with ice is no new one. He watched it years ago in Vienna and was impressed by the fact that it relieved pain, but did not influence the progress of disease in the slightest degree.

DR. M. V. BALL said that in croupous pneumonia in children between the ages of four and twelve he has found the cold treatment very useful. How it acts is uncertain, scarcely by limiting the course of the disease, although it does seem to relieve, pain and children sleep well under it, while there is no difficulty in applying it.

DR. THOMAS J. MAYS said, in regard to the differentiation between catarrhal and croupous pneumonia as they stand related to the treatment with cold, that in his earlier experience with the ice-bag he confined it exclusively to cases which were affected with the croupous variety of this disease. He soon found that others, among the first of whom was Dr. Franklin, of Hightstown, New Jersey, used it with equal success in the acute catarrhal variety following measles, etc., and since then Dr. Mays has used it in all cases of acute pneumonia, unless there existed some possible contraindication.

It has been well said that fever is no index as to the severity of the pneumonic process, although practically this point does not receive general recognition. Thus there may be very high fever with very little lung-involvement, and on the contrary, a large area of pulmonary disturbance with little or no fever. It must be remembered, however, that the degree of temperature present may not always represent the degree of thermotaxic disturbance.

Concerning the influence of cold on the pneumonic process it is very difficult to say anything definitely so far as the *modus operandi* goes, yet every observer will bear testimony that it possesses a direct influence on the local process. This was the experience of Dr. Lees, of London, who wrote on this subject before Dr. Mays did, and from whose work the latter received the inspiration which led him to test this treatment; and it has been seen over and over again by a number of other practitioners. It not only limits the pneumonic process, but in many cases when it is applied early enough it cuts short the disease.

In regard to the danger of fever Dr. La Place hit the nail on the head when he said that pneumonic fever develops the toxins referred to in the paper, and which have the power of producing convulsions, stupor, and other morbid nervous manifestations in the lower animals. Is it wise, therefore, to sit passively by and allow these agents to disintegrate the nervous system as far as they are capable of doing so, or is it not the physician's duty to make every possible effort to reduce the fever and thus check and annihilate this process of devastation? As has been said, it is well known that a temperature of 104° is not dangerous, at least not dangerous to life in most cases, yet to say this seems to be somewhat apologetic; for most physicians welcome a fall from 104°, rather than see the temperature remain at this point, and if intelligent efforts fail in bringing about a reduction, the fault does not lie so much in the physician as it does in the disease. While fever may be a good thing for the disease it is a very poor and dangerous experiment for the patient.

Nor does the coat-fitting argument apply to this case; for no one advocates the indiscriminate use of cold in pneumonia. There are certain characteristic morbid conditions which are known and recognized as pneumonia, and while it is true that cold does not fit every case in which such a collection of symptoms obtains, yet those experienced with cold know that it entirely fits the great majority of such cases; and those in which it is not admissible must be regarded as exceptions to the general rule. The same objection may be leveled against the external use of cold in typhoid fever; yet as a rule no one at present thinks of treating this disease without the use of cold-water baths.

The other objection that has been raised that the local is not superior to the general application of cold in pneumonia certainly gainsays the experience of Dr. Mays and that of every one else who has made a comparative study of this question. That general bathing is useless in this disease is not asserted but when it is desired to get the best effects of cold in pneumonia its influence must be concentrated on the local condition in the lung.

To the comparatively small number of cases of osteomyelitis, due to infection with the typhoid bacillus, in which the identity of the organism found was well authenticated, Bruin (*Annales de l'Institut*, Pasteur, April, 1896) contributes another. The bone affected was the left tibia, which contained a cavity filled with granulations and a small quantity of bloody pus. From the material removed at the operation a bacillus was isolated which proved itself to be identical with the bacillus of typhoid fever, after having been subjected to a careful study. A remarkable fact about the case was that the patient had had typhoid fever six years previously and an osteoperiostitis of the femur of the other limb during convalescence. There was also a history of pain in the left leg about the same time, which gradually quieted down, but had in latter years recurred at intervals. This case seems to show clearly that the typhoid bacillus can survive in bone for an indefinite period without necessarily causing very serious disturbance.—*Boston Med. and Surg. Journal.*

PERISCOPE.

NEWS AND MISCELLANY.

The connection between psychology and some physical defects of childhood and the bearing of this relationship on education was discussed in a recent number of *Pediatrics*. The good service rendered by school teachers in observing the mental peculiarities of the children under their care is illustrated by instructive instances. Among these may be noticed that of a school-mistress, who noted, without professing to explain, the inability of a large number of boys to spell correctly some of the simplest words. It appears to have been clear that this result was not attributable to mere ignorance, but rather to the wide prevalence of a degree of verbal deafness. If this explanation be correct it is evident that deafness in childhood and youth is much more common than has hitherto been suspected. Other cases referred to suggest the conjunction with deafness of a form of verbral amnesia, words being occasionally heard and even pronounced by a mute under exceptional conditions, but as surely forgotten. On the strength of these and other like observations the writer proposes the addition of special schools to the existing educational system for the purpose of meeting the difficulties of such cases. Probably a reform so radical is not sufficiently justified by the necessities of the case, and the responsibilities and expenses connected with gratuitious instruction are in all countries heavy enough already. What these investigations, crude though they be, do illustrate is the facility in observing many minor, but still significant, defects in childhood which belongs to the school teacher and the advantage of using this power as a means, not only of directing education, but roughly diagnosing physical or mental failings at an early stage while they are still in many cases well within the reach of remedial treatment.—*The Lancet.*

The question of how long a healthy person can remain without sleep has been experimentally investigated by two medical men attached to the University of Iowa. Somehow the inquiry seems to have just a little appropriateness when taken in connection with the arduous electoral campaign, the termination of which has just been reached by the election of a new President in the United States. However, Drs. Gilbert and Patrick, the two medical men in question, determined, without reference to other matters, to ascertain for themselves the longest period which they could remain without sleep. The result of their experiment was that they were able to keep awake continuously for four days and three nights. During that time they never slept once, but this was all that they were able to accomplish. At the end of the period mentioned they had arrived at such a serious condition of prostration that it would have been only at considerable risk had they attempted to prolong the experiment. Consequently they were compelled to desist, with the result that when they allowed "Nature's sweet restorer" to come to their aid a good night's rest was all that was requisite to relieve them of their fatigue. We cannot see, however, that their experiment has added very much to the scientific knowledge of this progressive century. Sleep is one of the gifts of a merciful Providence to man, and as such has become a law of Nature. To attempt, therefore, to ascertain to what extent the law can be broken without mishap does not appear to be a very logical proceeding, or one from the investigation of which any good is likely to ensue.—*Medical Press and Circular.*

A rare form of vicarious menstruation has been observed by Dr. Dswiemcinski, (*New York Medical Times*) where a female, menstruating first in her eighteenth year, married at the age of twenty-four. After a childless marriage of ten years her husband died. Three months before his death her menses disappeared for the first time, while at the same time a colossal swelling of the right mammary gland appeared in their stead. A large quantity of colostrum could be pressed out. At the same time there were pains in the breast which radiated into the arm. These symptoms lasted for three days and gradually disappeared, to reappear every month in place of the regular menses. The left breast was unaffected. He has observed this peculiar phenomenon six times. Dr. Cook, who was with the Peary expedition, says the Eskimo do not begin menstruating till they are eighteen or twenty years old, yet they have children at a much earlier age, and suggests that this is a proof that there is no necessary connection between ovulation and menstruation.—*New York Medical Times.*

NAPHTHALENE IN THE TREATMENT OF DYSENTERY.

℞

Naphthalene	15 grs.
Calomel	8 grs.
Essence of bergamot	3 drops.
Sugar	a sufficiency.

M. Divide into ten wafers. One to be taken every hour.—*Semaine Medicale.*

TYPHOID FEVER—

Salol	1 dr.
Thymol	36 grs.
Bismuth subnit	2 to 4 drs.
Mucilag. acaciæ	2 ozs.
Syr. tolutan	4 ozs.

M. Sig.—Tablespoonful three times daily.—*Memphis Medical Monthly.*